

Psychological Review

EDITED BY

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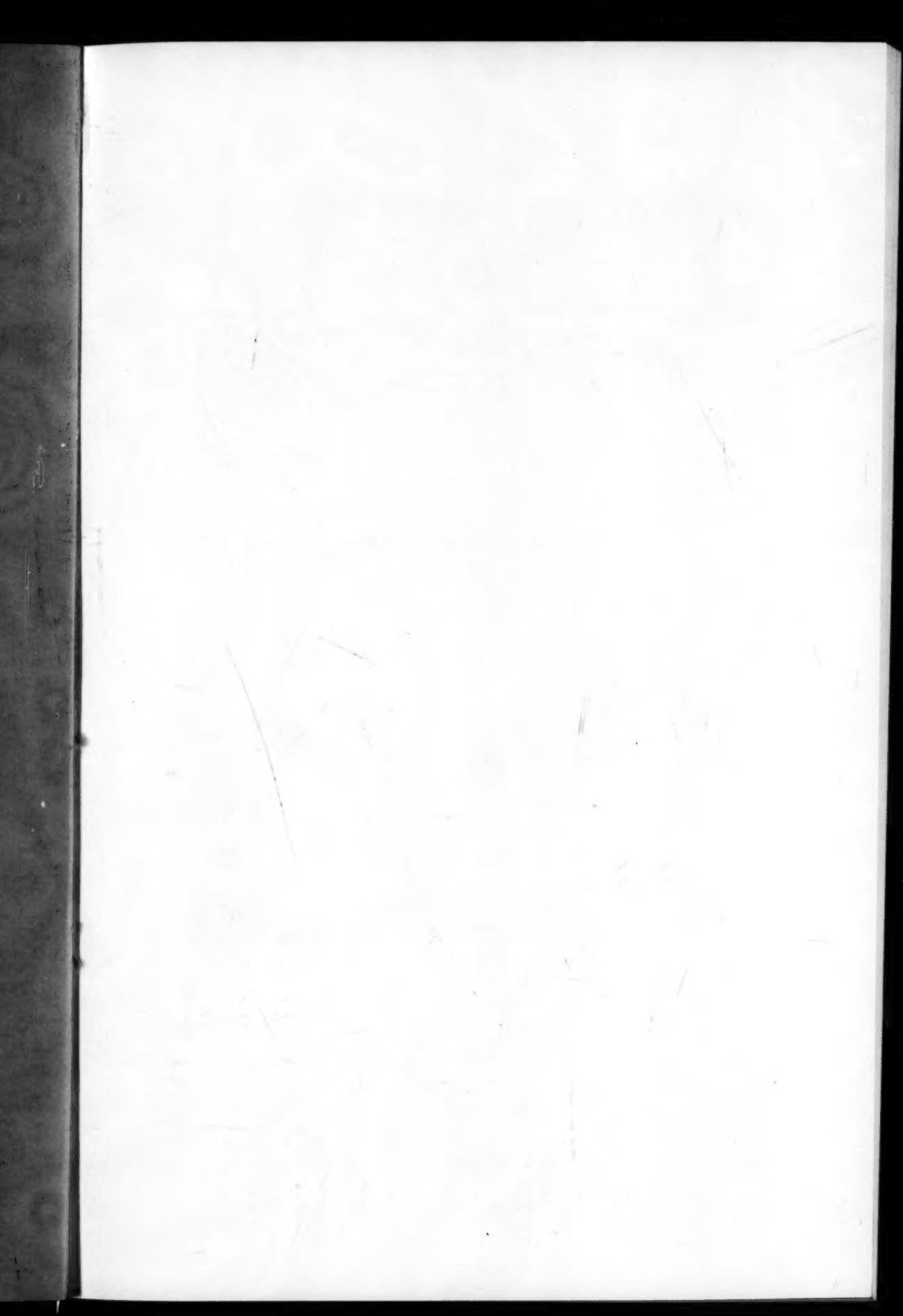
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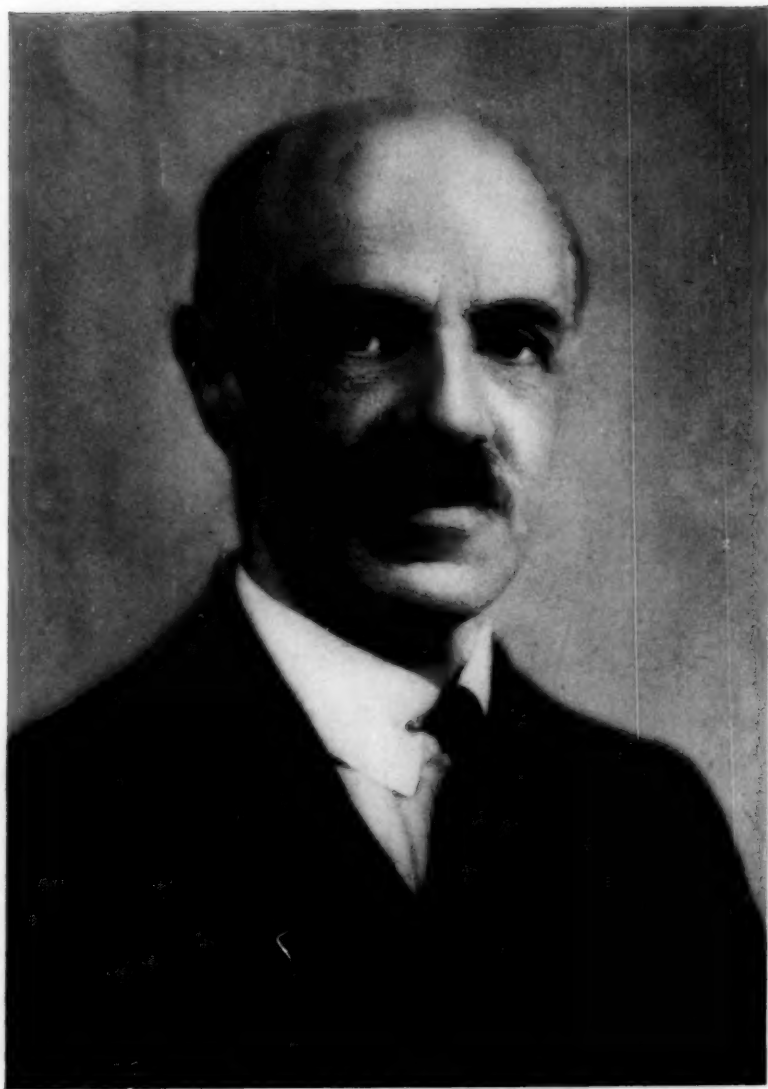
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C. S. Plarman

THE PSYCHOLOGICAL REVIEW

CHARLES EDWARD SPEARMAN

1863-1945

Charles Edward Spearman was born in London on September 10, 1863. He died there in University College Hospital on September 17, 1945, in his eighty-third year. Educated at Leamington College, he records that even in school days he felt an 'excessive but secret devotion to philosophy.' Partly to have an opportunity of studying Indian philosophy at first hand, he decided to undergo a brief period of military service. He obtained a commission in the Royal Munster Fusiliers and in due course passed the Staff College and served with distinction in the Burmese War of 1886, being awarded a medal and two clasps. To his profound regret, his military service lasted longer than he had expected. A small collection of philosophical and psychological books always accompanied him from one military station to another. It was not until 1897, when he was 34 years old, that he resigned his commission. By then he had become convinced that philosophy could advance only by way of psychology.

Spearman describes how, even before entering the Army, he had devoted himself to the aim of overthrowing "the dogma that all thought consists essentially of 'images.'" He describes how, after 20 years of 'patient though desultory' work on imageless thought, he discovered that Husserl had already anticipated him. Indeed he was always discovering how modern problems that attracted him, *e.g.*, that of the cog-

nition of relations, had been anticipated and successfully treated by the 18th century philosophers or by St. Thomas Aquinas and the Scholastics. By the time he left the Army he had already heard rumors of the new experimental study of psychology, and was attracted to the first laboratory of experimental psychology at Leipzig. Wundt was then little seen in his laboratory. Spearman learned the technique of his subject from Wundt's assistants, Krüger and Wirth, whose ability and personality he came to admire greatly.

But the Boer War of 1900 interrupted his Leipzig studies. He returned from Germany to Army life, serving partly in England, and in 1901 in Guernsey as Staff Officer. In Guernsey he met his future wife, Fanny Aikman, whom he married in 1901. She bore him five children; their only son was killed on active service in 1941. While he was at this time in England, Spearman examined a number of school-children in discrimination of pitch by means of a 'musical dichord' of his own contrivance, and found considerable correlation between discriminative sensibility and the children's school marks in various subjects.

His first interests at Leipzig appear to have centered chiefly in the psychophysical methods and their application to the determination of sensory thresholds. He published papers on "The Method of 'Right and Wrong Cases'

without Gauss's Formulae" (5), "Normal Illusions in Spatial Perception" (his Doctorate thesis in 1906), and "An 'Economic' Theory of Spatial Perception" (4). However, coming to the laboratory with a mature experience of practical affairs, he quickly felt that the experimental psychology of those days was too exclusively engrossed with processes far removed from everyday existence. His early aim, therefore, was, in his own phrase, to 'connect the psychics of the laboratory with those of real life.'

He and his collaborator Felix Krüger were much impressed by the studies carried out by Kraepelin and his pupils, particularly by Oehrn's work on 'Individual Psychology,' which he describes as 'the earliest actual experiments in mental correlation.' Oehrn, it will be remembered, tested ten subjects in various elementary functions, and concluded that perception, memory, and the motor functions are proportional to one another. At Paris Binet was engaged in a similar endeavor. But Binet's first investigations in this direction—seeking to determine what particular mental processes were associated with intelligence—struck Spearman as inconclusive because of their lack of a sound statistical basis. In America, Cattell, Thorndike and Wissler were following Galton's suggestion of combining a correlational technique with the technique of mental testing. This had proved disappointing, and had apparently demonstrated that "laboratory mental tests show little inter-correlation."

This negative result Spearman believed was probably due to an inadequate elimination of irrelevant factors. Consequently he proposed several formulae for the elimination of observational errors and other irrelevant factors on the lines of the work on partial correlation by Karl Pearson and Udny

Yule. Correcting the observed correlations by these supplementary formulae, he obtained much higher correlations both between tests of elementary cognitive processes and between marks for various school subjects.

The same formulae were used to demonstrate the identity of general intelligence (assessed by teachers, school marks, and fellow pupils) with general sensory discrimination (assessed by simple laboratory tests); for on eliminating the irrelevant or 'specific' factors in each, he obtained a theoretical correlation of approximately 1.00. From this he deduced "the profoundly important conclusion that there exists something we may provisionally term 'General Sensory Discrimination' and similarly a 'General Intelligence'; and the functional correspondence between these two is not appreciably less than absolute" (3, p. 272). Galton and other writers who had used a correlational technique had attributed the presence of a correlation between a single pair of intellectual processes to a hypothetical factor common to both. Spearman thus considered he had demonstrated that the factor common to *any* pair of intellectual processes was always the same. In this way he reached his theorem of the 'universal unit of intellectual function.' "All branches of intellectual activity have in common one fundamental function, whereas the remaining or specific elements seem in every case to be wholly different from that in all the others." By squaring the corrected average correlation he believed he could measure 'the ratio of the common factor to the specific.' And thus was born what Spearman later christened the 'theory of two factors.' In a paper published jointly with Felix Krüger he brought further evidence in support of these views, and suggested that this central function might be regarded as having

a physiological basis, and as resting on individual differences in 'the general plasticity of the nervous system' (2).

In 1907, after a brief period of study under Külpe at Würzburg and under Müller at Göttingen, he returned to England. At that time McDougall was particularly interested in Galton's scheme for an anthropometric survey which should include tests for intelligence; and, being greatly impressed with Spearman's work, McDougall obtained for him the position of University Reader in Experimental Psychology and of head of the small psychological laboratory which McDougall himself had started at University College, London, and was about to devote himself wholly to developing psychological work at Oxford. In the Galton Laboratory at University College Karl Pearson had already been working with correlational methods on teachers' assessments of mental characteristics among school-children. Spearman held that teachers' own impressionistic assessments were too unreliable to serve as the basis for scientific work; he proposed to encourage them to use tests of a laboratory type—the aesthesiometer, the dichord, the Galton cartridge weights—and to train them in the use of a simpler arithmetical technique for calculating correlations. To this end he devised his 'rank' formulae for correlation, which led to a vast increase of psychological investigation by younger research students and at the same time to forcible but not unfruitful criticism from Karl Pearson and his statistical colleagues. Meanwhile McDougall and others had suggested that even better assessments could be obtained if tests could be devised for the 'higher mental processes' involving more complex cognitive activities than simple discrimination. Such processes lent themselves more readily to group-testing, so that pupils could now be tested in much larger samples and probable errors re-

duced. This in turn led to suggestive discussion on the relative merits of group and individual testing, on the improved techniques for estimating the influence of the common factor, and on the relative importance of subsidiary 'group factors,' common to limited groups of mental processes only.

The problems suggested by all these incidental controversies stimulated Spearman and his pupils to a series of remarkable studies on the 'general factor,' where Spearman showed himself at once ready to incorporate modifications into his theory if he deemed the evidence sufficient, and equally vigorous in rejecting criticisms when he believed them to be ill-founded. Many still remember the exciting debates between Spearman and his younger critics at the tiny meetings of the British Psychological Society, when there was great competition for chalk and blackboard space. Probably this reciprocal enthusiasm did much to arouse the tremendous eagerness of the junior members of the audience to carry on further investigations of their own on one side or the other. The number of papers issued in bound annual volumes from the laboratory during the next 25 years was a remarkable tribute to the keenness and loyalty which Spearman inspired among investigators coming from all quarters of the globe.

Spearman himself was interested not so much in practical applications of mental tests as in the need for establishing first of all a sound knowledge of the structure of the mind to replace the old faculty psychology, and to furnish the new science of the mind with a series of 'basic' laws. With great originality and ingenuity he devised a number of new statistical tests for verifying his main hypothesis, and for meeting criticism from various quarters—from Karl Pearson and his pupils in the same College, from William Brown and Godfrey Thomson in other parts of the

country, and from Thorndike, Kelley, and their followers in America. Gradually, however, most of his early critics appear to have come very near to accepting Spearman's essential hypotheses—at any rate in a modified or extended form.

In 1911 he became Grote Professor of Mind and Logic in succession to Carveth Read. In 1928 this chair was re-named 'Professorship of Psychology.' On the outbreak of the first World War in 1914 he returned once again to the Army and joined the General Staff on the Tyne defences. Work on mental factors was suspended, and the laboratory at University College was mainly given over to psychological researches connected with the war itself—studies of night vision, of auditory discrimination in submarine warfare, of tests for flying personnel.

After the war was over, Spearman resumed his investigations on the 'general factor' from a somewhat different standpoint. Work on higher mental processes had meanwhile shown that more complex tests gave better correlations with intelligence than tests of simpler sensory processes, and that tests involving the perception of relations furnished the highest correlations of all. Spearman readily accepted these conclusions, and modified his original view which had identified the general factor with sensory discrimination. Instead, he now suggested that it was essentially a relational activity, discrimination being only one lowly form. He has explained how, during the war, he was led to meditate upon these further possibilities, and on his return to the lecture room he gave his first formulation, remarkably complete, of his new 'noegenetic principles of cognition.' This, he considered, constituted a 'Copernical revolution' in psychology. These ultimate principles included three qualitative and five quantitative 'laws.' The first three stated the principles of 'the

apprehension of experience,' 'the education of relations,' and the 'education of correlates.' The five quantitative principles were the principles of mental energy, retentivity, fatigue, conative control, and primordial potency.

The new principles, together with the work of his research students, were later summarized in his book on *The Nature of Intelligence and the Principles of Cognition* (6). This in turn was followed a few years afterward with a second book on *The Abilities of Man* (7). In these books and in other articles he developed an alternative physiological explanation of the central factor, *g*, which he now proposed to identify with 'cortical energy.'

He held that his theories of the nature of the general factor should cover the whole of mental life, and endeavored to demonstrate this point of view in what is perhaps the clearest account of his own doctrines, 'A School to End Schools' (8). Here and in other papers he recognized the value of the suggestive experimental results achieved by the Gestalt School, but held that they should be explained, not by invoking a new principle of Gestalt, but rather by applying his laws of the cognition of relations.

Later, in a smaller work on *Creative Mind* (9), he endeavored to show how these fundamental noegenetic laws might be applied to aesthetics and other intellectual fields. In that year he retired from his post at University College and received the title of Emeritus Professor. This left him free to accept several invitations to lecture in America. Before that date his researches, although at first published in an American journal, had attracted comparatively little attention in the United States. Kelley, after spending some time in England, had published a suggestive book, *Crossroads in the Mind of Man* (1), which to some extent seems to have aimed at combining the

views of Spearman and the 'two-factorists' with those of his critics, the 'multiple-factorists.' Consequently there were a number of younger psychologists in America eager to hear more about factor-analysis at first hand from its various British exponents. During his later visits to the United States he was able, in collaboration with Holzinger (who had already collaborated with him in several statistical publications) and with other American investigators, to plan a series of still more extensive investigations for verifying the 'two-factor theory' by a 'bifactor technique.' His last work, *Psychology Down the Ages* (10), is a monumental study of the evolution of the chief doctrines of psychology from the earliest times to the present day, with the intention of showing how all the acceptable formulations were really dim foreshadowings of the fundamental noegenetic laws. It had been begun fifty years ago, when he was saturated with the teachings of the Scholastics.

In 1924 Spearman was elected a Fellow of the Royal Society; later he received an Honorary LL.D. from Wittenberg, U.S.A., and was honored by scientific societies in Germany, France, the United States and Czecho-Slovakia. All who worked with him, or discussed with him their common problems, will readily testify to his remarkable gift for inspiring enthusiasm both among his own colleagues and pupils, and among his critics. Anyone who came to him with an earnest question or criticism was assured of a most patient hearing and of untiring exposition and assistance. No one can ever have spent more time in individual tuition, aiding younger investigators and correcting their immature accounts of their own inquiries. Few have possessed his gift of coördinating and concentrating the research-interests of pupils from the most different parts of the world on one single dominating and fertile theme. Spear-

man preserved his physical and intellectual vigor to the end; and even at the age of 80, when the Foreign Associateship of the National Academy of Sciences was conferred upon him, was still ready to aid others by correspondence or discussion. As was well said of him in *The Times*, "In virtue of his novel applications of statistical methods to psychological problems and of his exceptional capacity to combine exact experimentation with profound theoretical interpretation, he is assured of a permanent place among the great figures of mental science."

CYRIL BURT

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AND

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THE PAVLOVIAN THEORY OF GENERALIZATION¹

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An explanation of generalization as a product of irradiation of nervous excitation was first developed by Pavlov in studies of conditioned reflexes to cutaneous stimuli. The original form of the theory depended upon a number of postulates: (1) Excitation of any point on a sensory surface is projected to a corresponding point on the surface of the cerebral cortex; (2) from this point on the cortex, excitation (or inhibition) spreads at a relatively slow rate, with decrement; (3) any point in a cortical sensory field which is excited during an unconditioned reaction becomes associated with that reaction; (4) the strength of association is proportional to the strength of excitation during conditioning. From these assumptions it follows that association of reaction with one point on a sensory surface will be accompanied by weaker association with adjacent unstimulated points, the strength of association varying inversely with their distance from the point stimulated. A stimulus will be 'generalized' to the extent that its potential variations are represented by spatial distribution of sensory excitation on the cortex (17, p. 186). Pavlov discussed the theory only in reference to touch localization and pitch discrimination, for which spatial projection of the sensory surface could reasonably be assumed, but he suggested that the theory might be developed to account for all cases of stimulus generalization.

Hovland (6) has pointed out that the conception of spatial irradiation of excitation in the cortex is inapplicable to the generalization of intensities of excitation, since the nervous variables in such stimulation are number of neurons excited and frequency of discharge, not spatial position. As a result of such criticisms the neo-Pavlovian school has discarded the physiological assumptions of Pavlov's theory but has retained a number of assumptions and interpretations of the conditioned reflex which, although meaningful and reasonable in Pavlov's formulation, become questionable or meaningless without his specific conceptions of neurological activity.

BASIC POSTULATES OF THE NEO-PAVLOVIAN SYSTEM

The explanatory system developed by the modern disciples of Pavlov is built upon two assumptions which are fundamental to the entire structure. These are: (1) association by contiguity, with or without the additional assumption of a law of effect, and (2) the irradiation or spread of effects of training. The system has been developed in an attempt to derive perceptual and behavioral organization from the supposedly simple facts of primary conditioning. It is assumed that there is little or no organization of the various aspects of a stimulus during initial conditioning. The whole mass of excitation coming from the situation to which the subject is exposed during condition-

¹ From the Yerkes Laboratories of Primate Biology, Orange Park, Fla.

ing is associated, helterskelter, with the reaction. "... all elements of a stimulus complex playing upon the sensorium of an organism at or near the time that a response is evoked, tend themselves independently and indiscriminately to acquire the capacity to evoke substantially the same response" (7, p. 498). "Whenever a reaction takes place in temporal contiguity with an afferent receptor impulse resulting from the impact upon a receptor of a stimulus energy, and this conjunction is followed closely by the diminution in a need, there will result an increment in the tendency for that stimulus on subsequent occasions to evoke that reaction" (9, p. 71). "If the occurrence of an operant is followed by presentation of a reinforcing stimulus, the strength [of association] is increased" (21, p. 21).

Although Pavlov's physiological theory of irradiation and condensation of cerebral excitations has been discarded, his followers have retained the conception of stimulus generalization as an association of the conditioned reaction with a range of stimuli beyond that used in conditioning, and the consequent postulates of a decremental spread of effects of stimulation along 'stimulus dimensions' and of a 'gradient' of habit strength proportionate to the intensity of cortical excitation during conditioning. For irradiation of excitation they have substituted a physically and physiologically meaningless spread of the effects of training along 'dimensions' of similarity of stimuli. "The reaction involved in the original conditioning becomes connected with a considerable zone of stimuli other than, but adjacent to, the stimulus conventionally involved in the original conditioning; this is called *stimulus generalization*" (9, p. 183). "Points on the stimulus continuum falling beyond the range of the stimuli involved in the conditioning process also rise above the reaction

threshold but in progressively smaller amounts the more remote they are from the central tendency of the distribution of the stimuli conditioned" (9, p. 196).

The effects of training upon the association of various aspects or elements of a stimulus complex are assumed to be independent, except as they are affected by stimulus generalization. Associations with the various elements of the complex act by algebraic summation of excitatory and inhibitory tendencies to arouse the conditioned reaction. Discrimination involves the independent reinforcement of association with one stimulus and extinction of reaction to another, ultimately discriminated (4, 7, 21, 22).² The patterning of stimuli (perceptual organization) is accounted for chiefly as association of independent elements with a common reaction, but with the additional factor of 'stimulus interaction,' which is the mutual reinforcement or inhibition of effects of stimulation produced by their mutual involvement in irradiation or stimulus generalization (9, 10).

The neo-Pavlovian account of stimulus generalization obviously offers no explanation of generalization although its proponents seem to suggest that it

² Pavlov's own discussion of discriminative learning was thoroughly confused. In differential conditioning he noted that when the negative stimulus was first introduced the dog was disturbed and salivary secretion inhibited. This he interpreted as discrimination. As differential training continued this inhibition disappeared and equally strong secretion was obtained with the negative as with the positive stimulus. The dog no longer discriminated. With further differential conditioning, response to the negative stimulus was again inhibited (16, pp. 118). In other words, differential training at first destroyed discrimination and later reestablished it. The discrepancy was explained by Pavlov as due to initial inhibition of secretion by the arousal of the investigatory reflex but he neglected to provide an explanation for the later inhibition of this inhibition, so the explanation raises more problems than it solves.

does. It is merely a restatement of the ancient 'law' of association by similarity; it provides no answer to the psychological problem of what constitutes similarity or how the generalized association is developed. Stimulus generalization is accepted as a fact from which other principles of behavior can be deduced. Interest in such a formulation derives from its use in attempts to explain, in terms of habit strength, various phenomena which have seemed to other investigators to require the postulation of reaction to relations between stimuli, and which have formed the basis for field theories as opposed to associationism.

Stimulus generalization has been accepted as a basic fact in studies of the conditioned reflex, but it is actually an inference drawn by Pavlov from the initial lack of differentiation of the conditioned reaction. When the conditioned reflex has been formed to one stimulus, it may be elicited by a wide range of stimulus objects to which the subject was not exposed during conditioning. This is the established fact but it does not necessarily mean that during conditioning associative connections are generalized to stimulus attributes which differ in any way from those of the original stimulus object. The reported observations on the phenomena of conditioning do not justify any such conclusion. They are subject to an entirely different interpretation which leads to a questioning of the fundamental postulates of the neo-Pavlovian system and a denial of the validity of the entire structure.

The alternative interpretation, advanced here, includes the following points: 1. The phenomena of 'stimulus generalization' represent a failure of association. 2. There is no 'irradiation' or spread of effects of training during primary conditioning. 3. The 'dimensions' of a stimulus series are deter-

mined by comparison of two or more stimuli and do not exist for the organism until established by differential training. 4. The 'gradient of habit strength' is a product of variable stimulus thresholds, not of spread of associative processes. 5. Differentiation of conditioned reflexes involves the redirection of attention to new aspects of the stimuli and the formation of new associations with these, and is not due to any 'concentration' of excitation or reduction in the range of association.

In previous studies (13, 14) evidence has been presented to show that in differential conditioning (or discriminative learning) associations are formed with only a limited number of the differentiating aspects of the stimuli and that the particular associations formed cannot be predicted or accounted for in terms of habit strength produced by repetitive reinforcement, as postulated by the neo-Pavlovian system. Innate organization and past experience of the organism restrict the effective aspects of the stimuli in such a way as to make the practice required for learning a wholly unreliable measure of the learning process. In the present study we shall show that the phenomena of 'stimulus generalization' are a product of this same restriction of association to limited aspects of the stimulus and that the supposed spread of effects of training is an artifact resulting from an inadequate analysis of what is actually learned during the process of conditioning.

EXPERIMENTAL TESTS OF IRRADIATION

Pavlov devised a series of experiments to test the irradiation of effects of training, chiefly the irradiation of inhibition. The careful analysis of his data which Loucks (15, 16) has made shows that they not only fail to demonstrate irradiation but are actually, for the most part, inconsistent with Pavlov's

interpretation of them. Bass and Hull (1) and Hovland (5, 6) have reported experiments interpreted as evidence for a gradient of stimulus generalization.³ Their experiments are open to the objection that they used adult human subjects for whom the stimulus series represented familiar relational sequences, that initial tests directed attention to the stimulus dimensions investigated, and that the methods do not clearly distinguish between habit strength and ease of discrimination. To demonstrate irradiation in primary conditioning the subjects must be inexperienced with respect to the stimulus dimension used, in order to rule out any tendency to identify a single stimulus as belonging in a familiar graded series or to use habits of relational thinking. Further, the indicator must distinguish clearly between habit strength and ease of discrimination.

Animals are best suited as subjects for such tests; if human subjects are used, the stimulus dimension must be unfamiliar to them. As indices of habit strength the regularity or intensity of reaction, reaction time, resistance to temporal decay as measured by practice for recall (savings method), and resistance to extinction have been advocated. It is not certain that these all measure the same thing, but the last two methods of measurement are less influenced by difficulty of discrimination than are the others, since procedures requiring close attention to the stimuli may be used.

A simple test of irradiation consists of training a group of subjects in a reaction to a single stimulus, then opposing that stimulus to another on the

same stimulus dimension and comparing the rates of formation of a discriminative habit when the reaction to the initial stimulus is reinforced and when it is extinguished by the differential training. The experiments reported below follow this plan and meet the requirements for a test of irradiation in other respects.

Tests with rats. (1) In each of the following sequences of tests four rats, without previous experience in discrimination tests, were trained on the jumping stand:

a. Positive to a circular white area 8 cm. in diameter opposed to a black card; that is, to jump to a white stimulus object of definite size, as it appeared to the right or left against a black background. Training was continued to 200 consecutive errorless trials. A second white circle 5 cm. in diameter was next presented with the larger circle and the animals were trained to jump to the original larger circle and avoid the smaller; the original reaction was *reinforced*. Differential reaction (20 consecutive errorless trials) was established in an average of 190 trials with 57 errors.

b. Positive to a 5 cm. white circle opposed to a black card, with training continued to 200 consecutive errorless trials. An 8 cm. white circle was then opposed to the 5 cm. one and the animals trained to choose the larger. In this case reaction to the original stimulus was *extinguished*. Differential reaction was established in an average of 60 trials with 18 errors.

c. Positive to a black card opposed to a 5 cm. white circle, to 200 consecutive errorless trials. Next positive to the 5 cm. circle when opposed to an 8 cm. circle. In this case the differential training *extinguished* the initial inhibition of reaction to the small circle. Differential reaction was established in 150 trials with 56 errors.

³ Experimental results on this point have not been consistent. Wickens (23) failed to demonstrate a gradient of stimulus generalization. Razran (18) finds generalization with different types of stimuli too variable to formulate under any simple laws.

d. Positive to a black card opposed to an 8 cm. white circle, to 200 consecutive errorless trials. Next positive to a 5 cm. circle and negative to the original 8 cm. one. Training reinforced the inhibition of reaction to the original stimulus. Differential reaction was established in 160 trials with 55 errors.

A slight preference which a few animals have for the larger of two stimulus objects was controlled by having the same stimulus object positive in each pair of the above tests (*a-b*; *c-d*). The first pair measures the spread of a positive reaction (irradiation and excitation), the second pair, the spread of a negative reaction (irradiation of inhibition). Averages are the following:

	Trials	Errors
Original reaction reinforced (<i>a</i> and <i>d</i>)	175	56
Original reaction extinguished (<i>b</i> and <i>c</i>)	105	37

(2) Four rats were trained to choose a white cross opposed to a black card and were given 200 trials of overtraining, averaging 1 error each in the 200 trials. Two of the animals were next trained to choose the original cross and avoid an X of equal size; the other two were trained to choose the X and avoid the original cross. The training scores, to 20 consecutive errorless trials, were:

	Trials	Errors
Initial reaction reinforced	220	85
Initial reaction extinguished	150	54

The results of these two tests are typical of eight others dealing with surface brightness, direction of lines, number, and various similarities of form, which it is unnecessary to report in detail. In every case a differential reaction was established more quickly when the training involved extinction of the initial reaction to a single stimulus than when that reaction was reinforced. The differences are statistically unreliable but are consistent in all 10 experiments.

Tests with monkeys. Two monkeys (*Ateles geoffroy*) were used in 4 tests. Stimulus objects were painted metal plates concealing two food dishes on a tray placed before the cage. The animals were first given 200 trials with a single stimulus object, removing it from the right or left hand dish to find food. They were then given differential training with the original opposed to a new stimulus object on the same stimulus dimension. One monkey was trained to choose, the other to avoid the original stimulus and in different tests the direction of training was alternated between the two animals to control individual differences. Size, surface brightness, inclination of lines and number of lines were used as stimulus dimensions. Table 1 gives the results of

TABLE 1

TRAINING SCORES FOR MONKEYS IN TESTS OF STIMULUS GENERALIZATION

++ original reaction reinforced; +- original reaction extinguished.

Continuum	Animal No. 1		No. 2	
	Trials	Errors	Trials	Errors
Size	35	14+-	39	17++
Brightness	221	100++	149	62+-
Number	200	97+-	174	60++
Direction	129	53++	129	53+-
Average of ++	140	57		
Average of +-	128	56		

the tests. The difference (12 trials, 1 error) is insignificant. As in the experiments with rats, there is not the slightest indication that continued training with one stimulus forms any stronger association with that stimulus than with another distant from it upon the same stimulus dimension.

Tests with chimpanzees. Two adults and two about 4 years old were used. None of these animals had been trained in any discriminative task before. The

TABLE 2

TRAINING SCORES FOR CHIMPANZEES IN TESTS OF STIMULUS GENERALIZATION

Two adults and two four year old animals, not previously trained in discrimination tests, were first given 200 trials with one stimulus, then trained to 20 errorless trials with two. ++ original reaction reinforced; +- original reaction extinguished. F, failed to improve in 500 trials.

Figures		Animals							
Original	New	Shorty		Vera		Scarf		Banka	
		Tr.	E	Tr.	E	Tr.	E	Tr.	E
Orange star	Purple square	F	F+-	400	164++	88	47+-	10	4++
White crescent	Green triangle	90	33++	90	36+-	93	75++	15	12+-
Yellow circle	Red circle	150	51+-	150	62++	73	28+-	2	2++
2 stripes	3 stripes	62	25++		F+-	151	85++	323	157+-
Large rectangle	Small rectangle	282	116+-		F++	88	37+-	69	45++
Large L	Small L	207	81++		F+-	134	58++	398	163+-
Average ++		124.4 ±	71.8 trials	57.6 ±	30.7 errors				
Average +-		167.4 ±	101.0 trials	71.9 ±	36.6 errors				
Difference		43.0 ±	123.4 trials	14.4 ±	47.7 errors				

tests were arranged as with the monkeys except that the stimulus objects were painted fronts of boxes to be pulled to the cage by stout cords from a distance of 150 cm. In initial training the chimpanzee was presented with a single food box bearing a constant pattern, pulled it in, and got food from it 200 times. A second box with a different figure was then added and differential training continued to 20 consecutive errorless trials. The chimpanzees are more difficult to motivate and more erratic than the monkeys and the range of their training scores is so great as to make any conclusions from them questionable. In the first three tests the stimuli differed both in form and color, with the object of presenting optimal conditions for association with specific characters of the stimuli rather than for generalization; the three later tests involved differences of number or size. Training scores are given in table 2.

In tests following the series presented in table 2, Shorty was found to be reacting on the basis of differ-

ences between the cords used to pull in the boxes, and when those differences were controlled he failed to show improvement after 500 trials with each of two pairs of stimuli differing in color and form. His entire training score is therefore suspect. Vera, the other adult, learned to discriminate colors but failed all tests in which discrimination of form was required.

The averages for all scores of all animals (exclusive of failures) are given in table 2. Larger numbers of trials and errors were required when the original reaction was extinguished than when it was reinforced but the differences, 43.0 ± 123.4 trials and 14.3 ± 47.7 errors, are entirely unreliable. The variability and inconsistency of the scores justifies the conclusion that any strength of association established by 200 positive reactions to a single stimulus is insignificant in comparison with other factors which modify the rate of learning.

Test with human subjects. The human adult has a vast fund of general concepts and tends to classify and rank

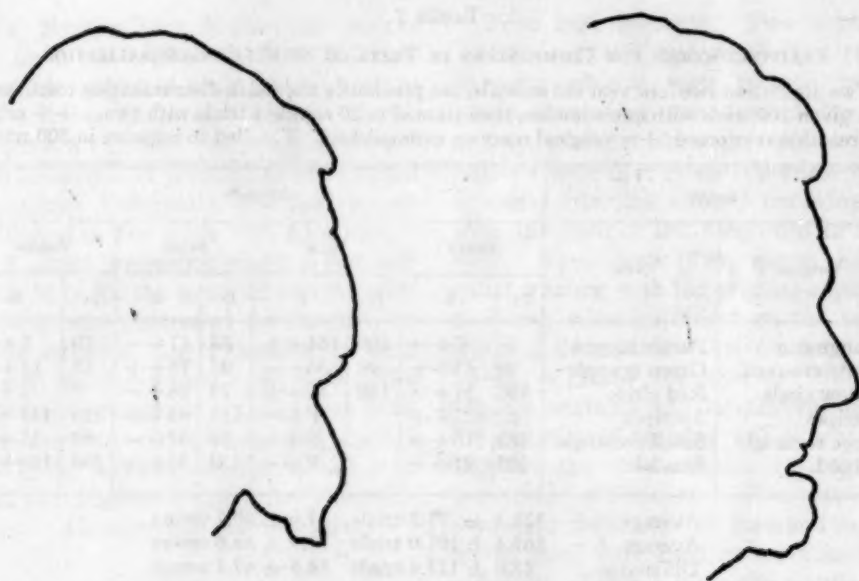


FIG. 1. Enlarged profiles from coins used in test of stimulus generalization.

any new object in one or another general category. Tests involving familiar stimulus dimensions are therefore worthless for evaluating the theory of irradiation. For a significant test of stimulus generalization a dimension must be chosen which is unfamiliar to the subjects, so that the formation of a new generalization can be observed.

Subjects were given a penny of current issue and a 'buffalo' nickel, asked to examine the coins carefully and to enumerate all the resemblances or dimensions of similarity by which the two could be compared and classified. Diameter, size, thickness, weight, color, value, dates of issue, amount of wear, and similar habitual categories of classification were readily specified by all subjects. No one, however, discovered the scale of similarity represented by the profiles enlarged in figure 1 until his attention was directed to it. The subjects had, of course, looked at coins of these issues hundreds of times before but such repeated exposures had failed to pro-

duce a generalization along the dimension of similarity of the profiles. With human subjects as with animals, repeated stimulation is not in itself sufficient to establish a stimulus dimension. The dimension arises as a result of attention to and comparison of two objects.

The results of all these tests are consistent and are incompatible with the theory of irradiation of the effects of conditioning. The theory postulates association with all aspects or dimensions of the stimulus and a maximum strength of association with the stimulus actually used in training. It therefore implies more rapid learning in those tests where the reaction to the original stimulus is reinforced than in those in which it is extinguished. No such result has been obtained. On the contrary, training with one stimulus fails to produce a significantly greater strength of association with that stimulus than with others on the same stimulus dimension. Under conditions which rule out pre-

existing habits of relational thinking, no evidence for any graduated spread of effects of training is obtained.

POSSIBLE OBJECTIONS TO THE EXPERIMENTAL TESTS

It might be argued that the amount of training with one stimulus was insufficient to establish a measurable difference in habit strength between it and other stimuli on the same dimension because the slope of the irradiation gradient is very slight. However, on the assumptions of neo-Pavlovian theory the 200 trials of initial training would establish a greater difference in habit strength between the positive and negative stimuli than that produced by the differential training which actually produced errorless discrimination.⁴ The

⁴We have not attempted to reduce these 'habit strengths' to the equations of the neo-Pavlovian system. The spurious character of its quantitative and mathematical treatment of learning is illustrated by the definition of its units of measurement, the hab and wat, in terms of percentage of the practice required by a 'standard' organism to reach the physiological limit of learning. Such units are completely devoid of meaning. The physiological limit is in no case determinable and is a concept of questionable value. Standardization is dependent upon one of three procedures. The standard may be an arbitrarily selected object which can be preserved unchanged, like the bronze bar representing the British standard yard. This method is feasible for a physical unit, but a standard rat might deteriorate seriously in the course of years. The standard may be calibrated in terms of some unvarying natural phenomenon, as the standard thermometer by the boiling and freezing points of water. No such natural constants exist for learning. The only remaining alternative involves the assumption of a correlation between different indices of the magnitude of the variable being standardized. In endocrine therapy a standard preparation may be defined in mouse units because this measure correlates satisfactorily with therapeutic effects. For learning, the correlation between different learning processes is so low as to preclude any such method; as well define the standard rat for discriminative learning by length of tail as by performance in the maze.

form of the gradient will not account for our results.

Lack of attention. It may be objected that none of the tests reported above is relevant to the problem because, during the initial training with one stimulus, the subjects did not attend to those aspects of the stimuli which were later differentiated. Spence (24) has advanced such a criticism against somewhat similar experiments of Krechevsky's. All of the tests required some manipulation of the stimulus objects by the subjects under such conditions that the stimuli were visually fixated and therefore excited the retina. The theory as originally proposed assumes that all afferent excitations from the stimulus are associated with the reaction. If this assumption is modified to the form, "all aspects of the stimulus which are attended to during primary conditioning are associated with the reaction," then the theory has to account for differences in attention during training with one stimulus and during differential training with two, when different aspects of the stimuli are associated.

Application of the theory to the problem of attention has been sketched by Spence (23, p. 432). "Moreover, the animal learns many other responses in addition to the final, selective approaching reaction. Prominent and important among these are what have been termed, for want of a better name, 'preparatory' responses. These latter consist of the responses which lead to the reception of the appropriate aspects of the total environmental complex on the animal's sensorium, e.g., the orientation and fixation of the head and eyes toward the critical stimuli. That is, the animal learns to 'look at' one aspect of the situation rather than another because of the fact that this response has always been followed within a short temporal interval by the

final goal response. Responses providing other sensory receptions are not similarly reinforced in a systematic fashion and hence tend to disappear." In other words, if an animal, in a situation requiring discrimination, attends to an attribute common to both the negative and positive stimuli, the attentive reaction to this attribute will be reinforced and non-reinforced, according to chance distribution of errors, approximately an equal number of times, and will be extinguished as a consequence of this irregularity of reinforcement. Its extinction will permit a shift of attention to another aspect of the stimulus, and to another, until one is hit upon which is characteristic of the positive stimulus only and can therefore persist as an object of attention.

This hypothesis is contradicted by the facts of conditioning. During primary conditioning, reinforcement on alternate trials is almost as effective for learning as is reinforcement on every trial (11, 12) and a well established conditioned reflex can be maintained at a high level of efficiency by only occasional reinforcement. Alternate reinforcement and non-reinforcement do not extinguish an established reaction, such as attention is assumed to be. The principles of reinforcement and extinction are inadequate to account for the direction of attention during discriminative learning.

Introduction of the question of lack of attention as an objection to our experiments would actually constitute an abandonment of the neo-Pavlovian system for it would discard the principle of association of all afferent excitations during conditioning (except on the untenable assumption that optic fixation can limit the kind of excitations aroused in the fovea, e.g., to excitations of form but not of color, color but not intensity, etc.), would deny the principle of 'continuity' in learning, which has been an

important conception in the development of the system, and would admit that stimulus generalization in primary conditioning is a function of limited attention and not of irradiation of effects of training.⁵

The nature of visual organization. Hull (9, p. 189) has implied that the complexity of visual organization is such that experiments involving vision are not applicable to the problem of stimulus generalization. "When numerous physical dimensions are mixed in various ways and, particularly, where interaction occurs between different parts of the retina, the nature and amount of the generalization effects are extremely difficult to predict." The same statement could be made with respect to any other sense modality. The conditions of auditory and cutaneous patterning are just as complex as those of visual. Tests with surface brightness, involving only one variable, give no different results from tests with patterns. If experiments on pattern vision are uninterpretable for the problem of stimulus generalization, so are experiments with all other sense modalities.

'STIMULUS GENERALIZATION' AS FAILURE OF ASSOCIATION

In the conventional method of establishing a conditioned reflex the animal is placed in an environment which is held constant throughout the experiment. A stimulus, a change in the environment, is introduced and followed by excitation of the unconditioned reflex. The conditioned and unconditioned stimuli are the only variables in

⁵ As a matter of fact, a mass of evidence which we cannot review here supports the view that attention to two objects in a situation is sufficient to establish an association between memory traces of them, and that the third recently adopted fundamental postulate of the neo-Pavlovian system, that association results from diminution in a need, is also fallacious.

the environment during the primary conditioning. If a human observer is asked to describe the stimulus to be conditioned, he enumerates a variety of aspects of the stimulus; it is light gray, of triangular shape, with base horizontal, with equal sides, of smooth texture, medium size, accurately painted, etc. The description is arrived at piecemeal by shifts of attention and every item involves comparison of the object with memories of other objects or with an habitual scale of measurement. During conditioning no such analysis occurs. For the animal something happens and then comes an electric shock. In the early stages of conditioning any change in the environment may elicit the avoiding reaction. Even with human subjects, conditioned to the sound of a bell, the senior author has obtained the conditioned reaction without further training from the sound of a buzzer, of breaking glass, of clapping hands, from a flash of light, from pressure or prick on arm or face.⁶ The only 'dimension' common to such stimuli is that all produce a sudden change in the environment. Such tests show that the conditioned reaction is initially undifferentiated; they do not tell what associations have been formed with the conditioned stimulus. The conditioning technique is not very satisfactory for analysis of the effective properties of the stimulus and no systematic study of what is actually associated during conditioning has been made. Our experiments suggest that, when a single stimulus is presented, reaction is associated only with the most conspicuous characters that differentiate it from the otherwise uniform environment (the stimulus is some *thing* that varies on a constant background). Analysis of the effective

⁶ This initial lack of differentiation has been called 'conditioned sensitization' by Razran (18). With the Bechterev method it has many characteristics of the startle pattern.

aspects of stimulation in discrimination experiments bears out this interpretation. After a discriminative reaction has been established, systematic variation of the stimuli always reveals that, of the many variables which differentiate the stimuli for the human observer, relatively few, often not more than one, are effective for the discriminative reaction of the animal (13, 14). The fundamental assumption of neo-Pavlovian theory, that in conditioning all aspects of the stimulus are associated with the reaction, is demonstrably false.

With continued training the subject may or may not develop reaction to a greater variety of aspects of the stimulus, may or may not show narrowing of the effective range on a stimulus dimension. Apparently such changes are a matter of chance noting of differences, generally with little regularity (19), although subjects may show some consistency in the order of difficulty inherent in different types of perceptual organization (3, 20). 'Stimulus generalization' is generalization only in the sense of failure to note distinguishing characteristics of the stimulus or to associate them with the conditioned reaction. A definite attribute of the stimulus is 'abstracted' and forms the basis of reaction; other attributes are either not sensed at all or are disregarded.⁷ So long as the effective attribute is present, the reaction is elicited as an all or none function of that attribute. Other characteristics of the stimulus may be radically changed without affecting the reaction. What is associated in any given case can be

⁷ The many reports quoted by Senden (20) of congenitally blind patients with vision restored by operation are consistent in showing that the first visual generalizations consist in the identification of isolated characteristics, color, the presence or absence of angularities, and the like, and that less conspicuous or differentiating properties of objects are completely disregarded.

discovered only by systematic variation of the stimulus and such an analysis reveals great individual differences depending upon innate tendencies to perceptual organization, the past experience of the organism, and emphasis on one or another attribute given by the experimental situation. No adequate evidence of a simple summation of the associative strengths of all stimulus attributes, such as is described by Hull (7), has ever been presented and there is ample evidence of the limited and selective character of association in which no such summation occurs. The stimulus generalization of conditioned reflex theory is evidently nothing more than this failure to observe and form effective associations with specific aspects of the stimulus.

Such generalization by 'default' presents a somewhat different problem from generalization which involves the definition of a stimulus dimension. If a monkey is trained to choose a *large* red circle and avoid a *small* green one, he will usually choose any red object and avoid any green but will make chance scores when like colored large and small circles are presented. There is no question of generalization here; the dimension of size is not seen as relevant to the situation. If, however, the monkey is trained with gray circles of unequal sizes, he not only differentiates the training objects but generalizes the reaction to size, as may be shown by transposition tests.

This definition for the animal of stimulus dimensions is a fundamental problem of generalization. It does not occur in conditioning to a single stimulus but is somehow a function of differential training with two or more stimuli on the same dimension. As we shall show in the following section, the dimension itself is created by or is a function of the organism and only secondarily, if at all, a property of the

physically definable character of the stimuli.

STIMULUS GENERALIZATION AND STIMULUS EQUIVALENCE

The postulate of irradiation has been advanced by Spence (23) as an explanation of transposition, or reaction to relative position in a unidimensional series, with the assumption of a gradient in strength of association in each direction along a continuum of similarities of stimuli. Of his explanation Spence writes, "... it is possible to deduce from stimulus-response concepts and principles that animals will respond to stimulus differences of degree in a manner which has hitherto been interpreted as involving a perception of a relationship or response to a structure-process (larger, brighter, etc.). According to the present hypothesis, however, the animal is responding in each situation to the particular stimulus object which has the greater excitatory strength. There is in the preceding account no assumption of a perception of the relational character of the situation" (23, p. 435). Hull (8, 10) has used the same assumptions in a more general theory which purports to explain all cases of stimulus equivalence. The apparent success of these 'explanations' lies in the inadvertent introduction of an assumption which begs the whole question of stimulus equivalence and introduces the very 'perception of the relational character of the situation' which the authors claim to have ruled out.

Pavlov attempted to explain degrees of similarity in terms of physical proximity of processes in the cerebral cortex. He assumed that the physical dimensions of the stimulus are translated into spatial relations within the brain, where the degree of similarity (*i.e.*, the readiness of confusion) is determined by the distance of separation of excited points

in the cortex, and the direction of generalization by the spread of excitation.

In discarding his physiological theory his followers have been forced to make the assumption that degree of similarity is a direct function of the quantitative physical relations of the stimulus objects. This assumption is not explicitly stated but is implied in all discussions where degree of similarity is expressed as distance on the physical continuum (8, 21). If it were true, there would be no problem of stimulus equivalence and the phenomena would be without theoretical interest. The assumption is, however, negated by every item of evidence. Degree of similarity is a product of the activity of the organism, not a physical property. When the physical dimension is quantitatively continuous, as the frequency of sound or light waves, confusions of discrimination may be more frequent for remote than for adjacent points on the continuum; violet is more similar to red than is green; the individual with absolute pitch more frequently confuses notes an octave apart than notes within the octave. Similarities in experience may exist for which no objective continuum is discoverable, as appears in the classification of odors and in equivalencies across sensory modalities. The relation of the physical to the organic continuum is not direct even in the cases of intensity and extensity, used as illustrations by Spence and Hull; Weber's law and the phenomena of size constancy are some of the many evidences that the physical dimension is always transformed by organic processes into a different dimension. This lack of correspondence between the physical and organic dimensions constitutes the problem of stimulus equivalence and generalization.

In a more recent discussion Hull has recognized the problem of the relation of the physical dimension to the 'af-

ferent generalization continuum,' corresponding to what we have called the organic continuum. This, however, leaves his discussion of generalization completely circular. "It is held that the number and nature of the various primary generalization gradients are caused jointly by the nature of the stimulus energy and the nature of the receptor response" (9, p. 198). No theory is proposed, however, to account for the production of a 'gradient of afferent generalization' by the receptor response. The statement quoted therefore merely constitutes an assumption that, through some mysterious process, the effects of training irradiate to stimuli which are similar for the organism. Similarity is not defined other than as a product of this irradiation. Nevertheless, this paralogism is advanced as a satisfactory solution of fundamental psychological problems. "In this way are explained both the paradox of the occurrence of super-threshold learning where the conditioned stimulus is never exactly repeated, and the paradox of reaction evocation where the evoking stimulus has never been associated with the reaction evoked" (9, p. 199).

Identification of degree of similarity with relative position of the stimuli upon the physical dimension or the postulation of a 'gradient of afferent generalization' introduces the conception that the organism reacts (projects the effects of training) to *relative* position and thus assumes the very reaction to relations to which Spence and Hull object in field theory. It actually goes even farther from their claimed objectivism than does field theory, for the latter requires at least two stimuli to determine reaction to a relation, whereas the theory of irradiation supposes that such a reaction is determined only by the subsistent or potential relations of a single stimulus.

THE GRADIENT AS A FUNCTION OF DISCRIMINATIVE THRESHOLD

A subject participating in an experiment on liminal differences may distinguish an eighth tone or less. The same subject, involved in a lively discussion, may fail to notice differences of an octave or more in the pitch of the voices engaged. A very squeaky or deep voice may distract attention from the discussion but the limen for differential reaction under such circumstances is enormously increased. In conditioned reflex studies the situation is not optimal for discrimination. Stimuli are given successively so that comparison is difficult; the subject is under apprehension of a painful stimulus and is concentrated on avoiding it. The unsophisticated subject reacts explosively to any change in the environment; the more sophisticated subject may limit reactions to stimuli which past experience has shown to be significant within the experimental situation, but still without close attention. Consequently a test for irradiation may give the appearance of a gradient of habit strength when it is actually measuring discriminative thresholds under distraction. The use of salivary or skin secretion as an indicator introduces an additional uncontrolled variable. These indicators are cumulative, build up gradually, so that increase in reaction time with difficulty of discrimination results in a graduated intensity of response. For these reasons the evidence for irradiation of training effects obtained under such conditions is inconclusive.

When animals have been trained in a discriminative reaction and are tested under conditions requiring close attention to the stimuli the interval between the stimuli may be decreased without producing the slightest disturbance of reaction until a point near the discriminative threshold is reached. Discrimi-

nation then suddenly breaks down and only long training reduces still further the apparent limen. When stimuli cannot be directly compared the range of uncertainty is increased. Thus when rats are trained to choose the largest of three circles with diameters in the ratio of 1:2:4 presented side by side on the jumping stand they first eliminate reaction to the smallest but continue to confuse the two larger, making mistakes however only when the two larger are separated by the smallest and are therefore less readily compared. When trained to choose the smallest they confuse the two smaller in exactly the same way, only when their separation is such as to make direct comparison difficult. When only two circles differing even less (ratio of 2:3) are used in initial training, discriminative reaction is established with less than one third of the training required by the three (13). The differences in rate of development of habit strength in these experiments are obviously due to differences in opportunity for direct comparison and not to differences in position on a gradient of irradiation. In those cases where positive evidence for a gradient has been obtained, experimental conditions were such as to make discrimination difficult. When inattention and threshold values are not involved, no evidence of a gradient is found.

It may be asked whether fluctuating liminal values do not themselves constitute evidence for a generalization gradient. In a sense they do, in that degree of sensitivity is graduated and measured along a stimulus dimension, but the fluctuation is a function of inaccuracy of perception and the dimension is determined by two or more points on the continuum. The gradient varies with the degree of attention and is unrelated to habit strength. The existence of such a gradient therefore offers no

support to the neo-Pavlovian doctrine of stimulus generalization.

CONCENTRATION AND DISCRIMINATION

Pavlov's theory of discriminative learning required a narrowing of the range of association, since he had assumed a wide range of association in stimulus generalization. He therefore postulated an inhibitory process, resulting from non-reward or punishment, attached to the negative stimulus during differential conditioning and irradiating from the focus of the unreinforced excitation. This was assumed to produce a narrowing of the excitatory gradient of the positive stimulus, a concentration of excitation, and hence a differential response.

Since his interpretation of initial associations is incorrect, the assumptions concerning concentration and the mechanism of differentiation become unnecessary. Analysis of the stimuli effective at different stages in the establishment of a differential reaction leads to the conclusion that differentiation consists in the successive establishment of associations with aspects of the stimuli which were not noted at earlier stages of training. Consistent failure following reaction to one aspect leads to more careful examination of the stimuli and reaction to another aspect, until one is discovered which leads consistently to success. The great individual variation with respect to the effective aspects of the stimuli points to chance factors in their selection and is irreconcilable with Pavlovian theory. Differentiation is not a reduction in the range of association but an extension of association to new elements.

CONDITIONS FOR THE DEVELOPMENT OF GENERALIZATION

Many instances of so-called generalization consist of nothing more than failure to observe differences. The objects

are placed in a category of things having a common trait. When whales were classed as fishes they were not considered less fish-like than other inhabitants of the ocean; their mammalian characters had not been observed. The development of a scale of similarity, the discovery of a stimulus dimension, on the other hand, requires both the recognition of similarity and of direction and degree of difference. To develop a gradient of similarity, comparison of two or more objects is necessary, either a direct comparison of sensory impressions or comparison of sensory impressions with traces of previous ones. Such a comparison establishes relational attributes which are just as fundamental as are any of the attributes derived from a single stimulus. It is, in fact, difficult to cite any stimulus attribute which is not dependent upon the integration of ratios of stimulus action. Size is a function of apparent distance, effective stimulus intensity varies with sensory adaptation and relative dominance of excitations from different sense organs, the direction of a line is a function of the observer's system of space coördinates. And so for every perceptual character, the effective attribute is determined by a ratio of stimulus intensities or positions.

In discrimination the *direction* of a difference is far more readily detected than are any absolute properties of the stimuli compared. Absolute properties may be retained in immediate memory but are difficult or impossible to transfer to a permanent record. A musician, lacking absolute pitch, may tune his instrument accurately within a few seconds after hearing concert A, but no amount of practice will make him competent to reproduce that pitch after a longer interval. Memory for pitch, brightness, intensity, hue and other quantitative variables behaves much as does the memory span for digits; it is

evanescent, markedly subject to retro-active inhibition, and even more difficult to fix in permanent memory.

A great deal of recent work has been devoted to study of alterations of the memory trace with passage of time. This has shown conclusively that memory for relations is far more permanent than is memory for absolute properties.

A mass of evidence, from the tropistic reactions of lower organisms to human learning, indicates that the basic nervous mechanism of integration is one of reaction to ratios of excitation. Generalization is one expression of this primary character of neural functioning.

Psychological analysis of perceptual similarity has apparently reached an impasse. No general laws descriptive of the processes by which a recognition of similarity is reached have ever been formulated. Perceptual similarity is psychologically as directly given as are sensory qualities. Both associationist and holistic systems have fallen back upon the conception of a gradient of relations between perceptions as the basis of similarity and generalization, but this gradient is in a purely hypothetical medium having no substantial relation to the nervous system or to the transmission of nervous excitation. It amounts to no more than a confession that the basis of similarity and the mechanism of generalization are problems whose solution depends upon the discovery of principles of nervous integration which are as yet completely unknown. It is important in this connection to recognize that nervous integration is primarily determined by ratios of excitation and that this principle provides a basis for the association of response to relations between stimuli, which is the essential character of generalization. At the present time nothing whatever is known concerning the nature of the alterations in the nervous system which constitute mem-

ory traces. Knowledge of cerebral physiology is in fact so limited that it does not even lend a greater plausibility to one than to another of the many speculations concerning the organic basis of memory with which the literature is burdened. Association with direction of flow of nervous excitation or with a ratio of excitation is neither more nor less fantastic as a physiological theory than is association between hypothetical conditioned-reflex arcs. The only relevant facts are those of psychology and the phenomena of stimulus equivalence and generalization are much more consistent with the former than with the latter alternative.

CONCLUSION

The neo-Pavlovian system of explanatory principles is built upon two fundamental postulates: (1) that in primary conditioning all stimuli which act during excitation of an unconditioned reaction tend to be associated with that reaction; (2) that effects of training with one stimulus irradiate to produce association with similar stimuli, with a strength of association proportional to the degree of similarity. Explanations of stimulus equivalence, of generalization, of 'afferent neural interaction,' and of perceptual organization or 'patterning' are based upon these two postulates. Both postulates are contrary to fact.

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EMOTION IN MAN AND ANIMAL: AN ANALYSIS OF THE INTUITIVE PROCESSES OF RECOGNITION

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I. INTRODUCTION

There has been a marked and necessary scientific reaction against the mentalistic extravagances of earlier writing on animal behavior. There is little justification and less explanatory value in ascribing man's elaborate conscious processes to animals, and discussing emotions in such terms would be futile. At the same time, a rigid refusal to 'anthropomorphize' may have its scientific disadvantages. Obviously, the true objection to anthropomorphism is not to discovering a similarity of mechanism in human and animal behavior, but to inventing similarities that do not exist. A complete rejection of all concepts derived from experience with man would leave a vacuum in animal psychology, for the closer we come to man in the phylogenetic scale the more evident it is that some quite complex modes of human behavior occur in animals and one cannot help recognizing that the overt pattern is the same in each case. A discussion of jealousy in the earthworm is obvious nonsense, but not in primates.

The difficulty extends throughout the field of emotion and temperament. Among the chimpanzees of the Yerkes Laboratories there are marked, stable differences of behavior which simulate all the varieties of human temperament. There are no names for these differing modes of behavior except anthropomorphic ones. In spite of its mentalistic flavor and connotation of reference to conscious processes, the anthropomorphic terminology in this field may have

another and more valuable significance as a classification of overt behavior.

A thoroughgoing attempt to avoid anthropomorphic description in the study of temperament was made over a two-year period at the Yerkes Laboratories. A formal experiment was set up to provide records of the actual behavior of the adult chimpanzees, and from these records to get an objective statement of the differences from animal to animal. All that resulted was an almost endless series of specific acts in which no order or meaning could be found. On the other hand, by the use of frankly anthropomorphic concepts of emotion and attitude one could quickly and easily describe the peculiarities of the individual animals, and with this information a newcomer to the staff could handle the animals as he could not safely otherwise. Whatever the anthropomorphic terminology may seem to imply about conscious states in the chimpanzee, it provides an *intelligible and practical guide to behavior*. The objective categorization therefore missed something in the behavior of the chimpanzee that the ill-defined categories of emotion and the like did not—some order, or relationship between isolated acts that is essential to comprehension of the behavior. The question then is raised whether one might start with the intuitive categories of emotion and discover by analysis what behavioral relationships they are based on. In this way one might objectify the categorization and make it suitable for the purposes of a scientific comparative psychology.

Such an analysis would have value in two other respects. It is worth while

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to show what is involved in the recognition of human emotions, since it appears that failure to understand the real nature of the process is responsible for much of the confusion in the field of emotional theory. It is also worth while to throw some light on the intuitive process itself. Intuitive judgments are to be found in use as scientific data in fields as divergent as abnormal psychology (in the recognition of psychopathic syndromes) and cytoarchitectonics (where an adequate definition of the characteristics of any of Brodmann's areas, except 4 and 17, has never been achieved). Whenever an intuitive judgment can be objectified it will be improved, for in so doing one decreases the likelihood of an illicit confusion between interpretation and the fact interpreted.

It is proposed therefore (1) to analyze the way in which human beings recognize chimpanzee emotions; (2) to show that the recognition of human emotions is on a similar basis; and (3) to show that references to emotion, attitude and so on, whatever they may seem to imply about conscious entities that may not exist, even in man, have value as summary descriptions and predictions of behavior.

II. THE USE OF TERMS

Several terms will be used in discussion which can easily be misunderstood, and it is necessary to define them explicitly.

Intuitive is used to refer to judgments which follow premises or steps of inference of which the judge is unaware and especially those which he cannot put into words. The word has undesirable connotations but there is no good substitute for it. The fact is evident that one frequently reaches a right conclusion without being able to state the evidence which really determines it. An equally evident fact is that intuitive judgments

are often wrong, and as long as they remain intuitive and un verbalized their flaws are not demonstrable.

Emotion is a term with more than one meaning. It is often used to refer to some distinctive mental state or conscious content, but sometimes it refers to more vaguely conceived states of excitation without any definite implications about consciousness. Now it happens, as we shall see, that competent students are agreed that there is no peculiar mental content referred to by the term emotion. In its most common use, therefore, the term has become meaningless. The second significance of the term seems more useful: at the time when an emotion is said to occur there is definitely a state of changed excitability or limen, with a selective effect on behavior. It used to be thought that the change of limen was due to a conscious event. This idea must now be rejected, but the fact of the changed responsiveness remains. It seems that 'emotion' could be used to refer to it. For this discussion, therefore, *emotion* explicitly does not mean any conscious process, nor any pattern of motor or glandular activity, but designates certain neurophysiological states, inferred from behavior, about which little is known except that by definition they predispose toward certain specific kinds of action.

Recognition of emotion then is the inference that one of these postulated states exists in the subject. It will be seen that there are two different conditions of the inference, which are mentioned here only to bring matters of terminology together. (1) In one condition, a deviation of behavior from a more usual pattern is directly classified as due to a particular emotion by the nature of the deviation itself. This is the recognition of 'primary' emotional behavior. (2) In the second condition, classification is determined by the fact

that the observer has recognized an accompaniment of the primary pattern: some act which would not in itself be classed as due to a particular emotion has become, by association, a sign of that emotion. The behavior in question is called 'associated' emotional behavior. The basis for this distinction will be shown later.

III. MATERIALS AND METHOD

The data upon which this study is based are the accumulated diary records of chimpanzees in the Yerkes Laboratories; descriptions of individual animals, made in their own words by four members of the scientific and caretaking staffs of the Laboratories; and systematic records (made in detail by a system of abbreviations) of the behavior of the animals—particularly at the times when a member of the staff was ascribing some emotion to the animal he was observing.

At the time of this analysis the Yerkes colony included 30 adolescent and adult chimpanzees, all but two of which had been under daily observation for periods of 6 to 19 years. For each animal a diary record is kept in considerable detail. It records behavior from the date of birth or of arrival at the colony, and often the staff's interpretation of the behavior worded in terms of emotion or attitude. Many such entries are based on intimate knowledge of the subjects.

It will appear from the subsequent discussion that this familiarity with the animals is particularly important. Current ideas about the recognition of emotion are based on the laboratory studies of the last 30 years, which have used human subjects and judgments based on a short period of observation. These studies have led to the conclusion that an emotion cannot be accurately identified by another observer.

However, it is possible that such a conclusion is the product of a particular experimental method. The chimpanzee data can give us information about judgments of emotion made in other circumstances, which would be hard to control experimentally with man but which in their time relationships are comparable to the recognition of human emotion in social situations.

For lack of space, the full details of recorded behavior upon which the following discussion rests cannot be given, except in a few instances which perhaps will illustrate all. The discussion really depends for conviction on the reader's social experience and upon his finding such an evident parallel between usage of emotional terms with chimpanzees and with human subjects that he will not need detailed illustrations in every instance. A much larger body of behavioral records and controlled observation has been analyzed than can be reported here.

IV. NAMING 'PRIMARY' EMOTIONAL BEHAVIOR

It has already been said that there are two conditions in which chimpanzee emotions may be named. One is the classification of a particular form of behavior as due to some emotion when the pattern of behavior in itself is enough to determine the naming (though the necessary temporal duration of the period of observation, of the pattern of behavior, may be long). The second condition is when the naming depends on the observer's having learned that the behavior he sees is frequently accompanied by intrinsically diagnostic behavior of the first type. The justification for the dichotomy appears in the following section; here we are dealing with the classification of behavior which is observed as forming a characteristic pattern.

The first fact to establish is that the

naming of chimpanzee emotional behavior is rarely based on the immediate behavior only, or even on immediate behavior plus a knowledge of the stimulus. The first most comprehensive example is found in the use of three terms relating to avoidance: *fear*, *nervousness* and *shyness*. The context of the diary references, and observations of behavior to which these terms were applied, show that the terms in general have definitely distinguishable references. *Fear* is used when a specific object or region is avoided and implies that the response may be a strong one. *Nervousness*, quite apart from an alternative reference to a long-term characteristic of the animal, is used to refer to a temporary lowering of the avoidance limen, but not to the point of constant or extreme avoidance. Sometimes this state of affairs is detected when a sudden movement or noise produces startle more easily than at other times. *Shyness* specifically implies that the object avoided is man or another chimpanzee, and that more familiar persons are not as strongly avoided. Both *nervousness* and *shyness* imply that there is no adequate stimulus to avoidance (that is, the objects avoided are not such that one need expect them to cause avoidance); and that the avoidance is not indefinitely prolonged, for otherwise the observer speaks of an unexplained 'fear.'

Two points emerge here. One is the definite importance of the kind of stimulus in the choice of terms by which to designate the response. The same thing has been emphasized in the recognition of human emotions. But this is not to say that the stimulus alone determines the choice, as some writers have concluded. The response itself, as an avoidance, has a definite part to play. The second point is just as important. The choice of terms is affected by familiarity with the animal: his past be-

havior, the amount of his exposure to various stimuli, and his responses to them. Saying that an animal is being shy, for example, not only says that he is avoiding an unfamiliar person, but also that he does not avoid familiar ones. Otherwise his avoidance would be called nervousness, or fear of man. 'Shy' is a summary reference to a variability of behavior with stimuli which are of the same kind except with respect to the subject's past experience. This is a further qualification of the idea that the stimulus is what determines the choice of names for an emotional state. In some instances, knowledge of the stimulus is important only because the observer's interpretation takes in the animal's experience and behavior with similar stimuli. In short, the distinction between *shyness*, *nervousness* and *fear* is impossible without knowing something about the animal; and the term chosen, in these instances, is affected by knowledge of the stimulus, of the response, and of the subject's experience and behavior in the past.

Let me repeat that what is discussed here is the designation of transient emotional states, although some of the terms mentioned may also refer to habitual or lasting modes of behavior. Obviously one would have to be familiar with an animal—to observe his behavior repeatedly—before one could state that he always avoided strangers. But the point is that shyness might be observed on a single occasion only, and naming it as shyness would still demand a knowledge of the animal's lack of avoidance of persons he knows. The same term, 'shy' or 'nervous,' can refer to an immediate emotional condition, or to the fact that the animal is in such states often. The distinction is the same as that between a nervous person and one who is nervous as he gets up to make a speech. In chimpanzees, the designation even of the temporary emo-

tional states requires knowledge of the animal's past behavior.

Another example is found in the naming of rage or anger. Bimba, one of the chimpanzees of the colony, is said to be friendly to man, but quick-tempered; while another chimpanzee, Pati, is said to hate man. Bimba's anger, and Pati's hate, are actually manifested in attacks which cannot be reliably distinguished from one another. But when Bimba is not attacking an observer, she behaves very differently from Pati. She is always responsive to man, and acts in a way which promotes contact and petting by the attendants (except at the times when she is said to be angry), while Pati has a long history of vicious attacks with few efforts to be friendly. It is in this difference that the real distinction of rage from hate must lie.

This was not evident at first. It seemed that there actually was a real difference in the way in which these two animals made their attacks. Due presumably to a kind of halo effect, Bimba's attacks seemed to occur only after some movement by the attendant that might appear like a threat, or like teasing; and it seemed that her attacks were always open, with the frank violence of anger, while Pati's were stealthy. But this was a fallacy of memory. When a record was kept of the circumstances of the attacks and their form, it appeared that Bimba, the friendly, made more attacks without cause than Pati; and that fairly often she attacked deliberately after getting the observer within reach by an appearance of friendliness. (The greater frequency of Bimba's attacks, however, is not significant; because she is so friendly she gets many more chances.)

There was then no distinction between some of Bimba's attacks and some of Pati's. Yet there was perfect agreement by the staff in regarding all

of Bimba's as due to anger and not to some fitful malice. The name given to a transient emotional state was determined in part by the immediate behavior (that is, by the fact that it was aggressive) but also by behavioral relationships over a period of years. It should be noted that the judgment was an intuitive one, the observers not realizing how much entered into their choice of terms. The person who does the identifying seems to feel instead that in some way (which he cannot specify) the immediate behavior in itself is really distinctive. In these circumstances, we can understand the skepticism of psychologists unfamiliar with the animals, when they are told that the same behavior is an expression now of one emotion, now of another.

The classification of Bimba's attacks as due to anger is also a particularly clear case of the way in which naming an emotion is occasionally quite independent of a knowledge of the stimulus. On repeated occasions there was no provocation at which one could even guess—that is, there was no stimulus evident. Yet one thinks of anger as precipitated by some event, real or imaginary. It is therefore significant that one of the caretakers said that Bimba is 'quick to resent slights, or a fancied lack of attention.' Another said that she 'gets mad over nothing at all,' meaning that the causes were trivial and not such that they should lead to anger. A psychologist would hardly dare to make formal use of such language but it can be said that the scientific staff acquiesce in the description, and if the statements are interpreted as they would be socially, with human behavior, they are a reasonable summary of the observed facts. Obviously the classification of the response is achieved first, and a thoroughly hypothetical idea of the cause, or stimulus, is set up to fit in with it.

If such constructs were made at random they would mean nothing. In order to show that the persons who diagnose Bimba's 'anger without apparent cause' are not making an entirely wanton use of language, several instances may be given in which the same persons are unable to interpret aggressive behavior, and refuse to name an underlying emotion.

Kambi is an animal who has been in the colony as long as Bimba, but the caretaking staff refuse in general to explain her occasional aggressions. Something necessary to a diagnosis is missing. There is a lack of the consistency in over-all behavior that leads to agreement in Bimba's or Pati's case. The staff simply regard Kambi as a 'screwball,' a moody psychopathic whose behavior is past accounting for.

Frank has generally been regarded as an exceptionally friendly and trustworthy animal. He consequently has unusually good opportunities to injure the attendants. Recently he made a deliberate and vicious attack, for the first time, taking two or three minutes to get a good grip on an attendant's forearm outside the wire mesh of his cage, in an apparently friendly way, before suddenly beginning to bite and gnaw at the flesh pressed within reach of his teeth. The incident has been repeatedly discussed by the staff, and circumstances have been mentioned which could possibly account for the attack; but the point here is that no one has definitely settled on an explanation, or categorized the behavior by specifying an underlying emotional state.

Finally, an adult named Shorty has been in the colony for two years. He is frequently aggressive, frequently friendly, and always responsive to man. No one, however, is yet willing to give a name to his attitude. Bimba's superficially similar behavior fits the conception of a friendly animal who is easily

annoyed; Shorty's does not. His aggressive behavior is too regular, and not even an imaginary provocation can be assigned. He becomes violent as an observer approaches and at these times may scratch or bite, but almost at once will change to a non-aggressive behavior if he is allowed to examine the observer's skin or clothing. He is tentatively thought to be 'friendly,' a term implying that in most circumstances one might enter his cage without fearing injury. But staff members are not at all sure of this with Shorty, as they are with some other animals, and no one is willing to classify his aggressive behavior by saying definitely that it is only done to attract attention.

These are examples of refusals to anthropomorphize. In general, the staff will refuse to designate emotional states in the animals more often than not. The instances which have been chosen for discussion, in which an emotion is named, might have suggested that such diagnoses were made indiscriminately. By making it clear this is not so, I am trying to show that there are definite prerequisites in naming animal emotions, even by the psychologically naive. The names are applied only when familiarity with the animal reveals a long-term pattern of behavior *with which the observer is already familiar in man*, and some evidence of validity in the categorization is found in its practical value in predicting the outcome of a behavioral sequence.

V. NAMING EMOTIONS FROM ASSOCIATED BEHAVIOR

The preceding discussion has maintained that there are temporal patterns of behavior that lead directly to the naming of emotions by human observers. These are referred to as 'primary' emotional behavior, meaning that from the observer's point of view the patterns are classifiable as soon as they

are clearly perceived. We come now to the way in which behavioral patterns which might mean nothing in themselves can determine a diagnosis of emotion, from their association with a primary emotional pattern.

In the chimpanzee, as in man, an emotional stimulus is not always fully effective but may produce a low level of excitation which is capable of summation, or of facilitating a later response. A single stimulation perhaps has a trivial effect. Prolonging or repeating it produces primary, characteristic emotional behavior, qualitatively changed from the apparently trivial response to first stimulation. The first stimulation may even have no evident effect, and yet have a facilitating action. These facts require the concept of subliminal emotional excitation, by precisely the same logic that allows us to deduce the various phases of excitability in a nerve cell.

Several examples can be given in which a stimulus, at first ineffective, gives rise to emotional behavior when it is repeated or prolonged. Cuba, caged next to Dita, begged loudly for some pieces of food which Dita had left uneaten. Dita gave her several pieces, then went away and sat in the sun; when Cuba persisted in her noisy begging, Dita at first sat watching her, then suddenly got up belligerently and beat violently on the wire separating them. In an experiment on avoidance a mounted snake was carried in the experimenter's hands up to the part of the enclosure where Shorty sat. He moved calmly away to another part of the enclosure, with no hint of excitement; but when the snake was again brought near him in his new position, he sprang up with hair erect and screaming, and hurled a large piece of timber at the experimenter with excellent aim. The same kind of summation effect is often seen in discrimination training,

where a single failure may have no apparent effect but repeated failures lead to sulking, temper tantrums or destructive attacks on the apparatus. In such instances the existence of a subliminal excitation is purely a matter of inference.

More often, however, the emotional excitation is subliminal only in a restricted sense; it does not produce a primary emotional response, but does affect the animal's behavior. A single experimental frustration may for a given animal produce the response of scratching, nothing else. Scratching is not intrinsically a sign of emotion; and in this sense the emotional excitation is subliminal. When repeated frustrations produce temper tantrums in the same animal, the scratching becomes an indication of a changed responsiveness, of an increased probability of temper tantrums, and it is assumed that the animal's emotional state is not the same as before stimulation.

In another animal, the preliminary signs of temper tantrums might be restlessness, or a noise like moaning. Neither behavior is in itself diagnostic, but knowledge of the animal and of the stimulus would make it diagnostic, by its association with temper tantrums when frustration is repeated. Individual signs of the development of frustration responses are quite varied. They include the animal's scratching himself, ducking his head, moving away from the apparatus quietly and becoming unresponsive to the experimenter, restlessness, moaning, whimpering, and erection of hair.

Within limits, associated behavior may have diagnostic value for a species as a whole. The erection of hair in a chimpanzee is in general a danger signal to anyone within his reach. Screaming on the other hand may be only a welcome for approaching food, and has not always the connotations it would have

with man. A sputtering noise with the lips is not derisive but usually a sign of friendliness and characteristically accompanies the grooming of another animal. Thus a knowledge of the species has considerable importance in the interpretation of the associated behavior of emotional excitation. Very often, however, any single item of behavior is not associated alone with a single emotion, but with several: erect hair occurs equally with rage and fear, and a particular posture by the female may precede copulation or be a signal of submission to another female. It is this fact, I think, that accounts for such concepts as 'nervous tension' and 'emotional excitement'—states in which it is evident, from associated signs, that there is emotional activity but with no definite indication of the specific form it will take. The significance of these phrases is apparently not invariable, but they seem to be much less frequently used when the observer has reason to expect a particular form of emotional behavior as the resultant of further stimulation. A more specific prediction of course usually depends on familiarity with individual characteristics. Important as a knowledge of the species is in interpreting the preliminary signs of emotion, the great variety of chimpanzee behavior appears to make a knowledge of individual animals more important.

The complexity of relationship which a neutral form of behavior may have to primary emotional behavior is illustrated by the unusual response pattern developed by the chimpanzee Soda in the course of an experiment with a number of avoidance-producing objects. The objects were brought close to the front of the animal's cage in a presentation box with a hinged cover which was opened for 30 seconds to expose the stimulus objects. Before and after exposure each animal was fed, with the

intention of bringing him close to the stimulus so that definite avoidance or lack of avoidance could be observed. In the early trials Soda showed a strong avoidance of objects which also produced avoidance in other animals, and was less excited by others which had not so much avoidance value. As the experiment went on, however, a peculiar stereotyped behavior developed. Soda always has a good appetite, and came regularly to get the food. As she got it, and while the presentation box was being opened, she would move promptly but without apparent haste to a point about 10 feet away and sit watching the infant quarters next door, her gaze at ninety degrees from the direction of the stimulus object. Once this mode of response appeared, only those objects which were very exciting for other animals could produce a clear-cut avoidance in Soda.

How was her later behavior with the less stimulating objects to be interpreted? For the purposes of the experiment, 'avoidance' was defined as moving directly away from the stimulus in such a way as to indicate that the response was produced by that stimulus: in the promptness and precipitancy of response, or by signs such as vocalization and erection of hair on the appearance of the stimulus, or by the combination of moving away from the stimulus and keeping the gaze fixed on it. Soda showed none of these signs clearly, except with the most stimulating objects, and in itself her stereotyped behavior on any one trial might as well be called a sign of boredom as of fear.

However, there are these points to be considered. The peculiar pattern of behavior occurred only in an exciting, and often a 'fear-provoking' situation; and Soda is known to be easily scared. The peculiarity of the response lay in the recognizable repetition of an otherwise trivial series of movements in special

circumstances, and in the alternation of these movements with another recognizable pattern of definitive avoidance. On a few trials, there were indications of haste in the stereotyped response itself. The stereotyped behavior, then, was associated with avoidance and with a special situation which produced avoidance in a number of animals. Furthermore, it was significant that Soda's avoidance limen was lowered even during her apparently calm observation of the infants next door. On two occasions the hinged cover of the presentation box was dropped by accident, producing a sudden but not loud noise. Each time Soda jumped back in a way that clearly conformed to the definition of avoidance. Such noises did not produce as strong a response at other times. In brief, Soda showed a kind of behavior which was distinctive only in the regularity of its occurrence in a particular situation; this situation was one which at times produced a violent avoidance in Soda, and frequently in other chimpanzees; and finally, at the times when she exhibited the stereotyped behavior her limen for startle responses was lowered. Her stereotyped behavior was associated with avoidance and an avoidance-producing situation, and by that association can be interpreted as a sign of fear.

In a somewhat similar experiment two young males, Tom and Dick, could see the experimenter approaching before their own behavior could be observed. The single stimulus used was repeated on a number of different days. At first it produced definite avoidance in these two young animals; later, however, when the experimenter arrived at their cage they were always found sitting quite calmly near the back wall of the cage. The later behavior did not fit the definition of avoidance at all; but its consistency in a situation which at first produced avoidance in these two ani-

mals, and which continued to produce avoidance in a number of others, was well beyond chance. A single observation could not give a diagnosis of fear, but the relationships observed over a longer period can.

These are instances of an apparent suppression or obviating of open emotional behavior. The chimpanzee is a very excitable animal, but it is not uncommon to observe such an 'inhibition' of fear responses among adult animals. The apparent inhibition occurs also with rage, but to a less degree, and is rarely observed with other emotions. It can be plausibly considered to constitute a conflict of motivations, in which behavioral associations sometimes give a clue to the presence of 'unexpressed' emotional tendencies. Far more often than not one can merely guess at the nature of an unexpressed emotion, however. Only in favorable circumstances can a definite interpretation be made.

VI. OBJECTIVE SIGNIFICANCE OF NAMED EMOTIONS

Let us now summarize the factors which have been shown to affect the choice of names for emotion in a chimpanzee, and ask how useful and acceptable such terms are for the purposes of comparative psychology. It is evident that the named emotion has a detectable objective significance, and that the apparent mental reference of the term should be disregarded (in view especially of the fact that emotional entities cannot be found even in human consciousness); but while these facts can be recognized and made use of, it would be wrong to leave the impression that nothing more is necessary to obtain a satisfactory categorization of emotional behavior.

Classifying emotional behavior (that is, naming emotions) is based on a complex set of cues. There may be in-

volved a knowledge of (1) the stimulus, (2) the subject's experience with this and related stimuli, (3) the response, (4) various aspects of the subject's other behavior and (5) behavioral characteristics of the species. Every judgment is not affected by all of these things, and the most important are (3) and (4), or the relation of the response to behavior at other times.

The primary emotional pattern is essentially a deviation from a base line which may differ with individuals. It is upon this that intuitive judgment puts weight rather than on the details of the act itself. Also, the intuitive judgment treats as equivalent all acts, however variable, which tend to have the same end result, implicitly postulating a constant central process with a variable manifestation.

Such a conception is not intrinsically improbable. Lashley (14) has shown that a highly variable motor pattern may be controlled by a single physiologically coherent mechanism, and his reasoning is directly applicable to emotional responses. The absence of a constant motor pattern in rage or fear behavior therefore is not evidence against the existence of psychological entities 'rage' and 'fear.' On the other hand, there is not sufficient evidence that our present classification of these entities as discrete, independently functioning processes is necessarily satisfactory. There is every reason to believe that there are innately established emotional entities, but the existing evidence does not take us much farther. It is one thing to show that a variable series of acts is under unitary control, particularly when one can demonstrate, as in sexual behavior, a correlation with a definable physiological condition; but it is quite different to show from behavioral evidence alone that a series A and a series B, occurring at different

times or in different subjects, are the product of a single mechanism and that this mechanism is discrete from others.

The various expressions of rage and fear, for example, might be so arranged as to represent a continuum, and only traditional thought can clearly dichotomize them. In other words, we have at present no good criterion for emotional classification. Certainly many of the distinctions of the traditional classification must be psychologically meaningless, referring, as they evidently do, not to a distinction of mechanism but to the circumstances in which the mechanism is activated or the degree of activation. One might guess that there are at least three different modes of emotional behavior—aggression, flight, and nonviolent adience—and perhaps more, but this must be only a guess until the actual physiological control of the various forms of behavior is understood.

The contribution of this paper is in showing just what objective facts underlie the traditional categorization, presumably facilitating the discovery of its elements of value, and in revealing the elaborate conceptual framework involved. It has been seen, for example, that the distinction of Bimba's anger from Pati's hate is not independent of the idea that Bimba is a friendly animal, and requires the conception of imagined causes of anger. It has also been seen that there is involved the postulate of subliminal emotional excitations; and frequently the variability of manifestation of an emotion is explicitly attributed to a conflict between emotional mechanisms. Such concepts may be right or wrong—the concept of a subliminal activation, for example, seems entirely justified—but there is no definite basis at present for separating bad from good. The traditional classification evidently implies an elaborate

theory. If there is some truth in it, as its practical value in predicting behavior suggests, the present analysis is a first step toward discovering what that truth is.

The unfortunate fact is that one's observations of emotional behavior are likely to be intuitive and a function of preconceived ideas, but following the direction of the present discussion it should be possible at least to retrace one's steps in the intuitive judgment and discover just what the facts are, as distinct from the concealed interpretation. In trying to isolate the cues which determine one's recognition of an emotion, the question to ask is not, "What is the distinctive feature of this behavior," but "What is the nature of the deviation from the animal's behavior at other times?" Then, "Is the deviation in itself enough to identify a particular emotion, or is it diagnostic only for the particular animal or species concerned?" This latter question makes the distinction between primary and associated emotional behavior. Finally, one may ask whether the same emotion would have been recognized if the stimulus had been different. With such an approach one can often determine with a good deal of confidence the cues which have affected one's judgment, and thus be in a better position to evaluate its significance. Before the thesis of this paper was clearly formulated, there were frequent occasions on which I could only say that a certain chimpanzee was afraid although he made no definable fear response—as in the case of Soda, mentioned earlier. When the distinction between primary and associated emotional behavior is made, and the course of events over an extended period considered, the evidence can be reported in a form which others can evaluate and not merely as an unanalyzable personal opinion.

VII. RECOGNITION OF MAN'S EMOTIONS BY AN OBSERVER

Here and in the following section I shall try to make the hypothesis plausible that naming man's emotions is much the same process as naming the chimpanzee's, and that the hypothetical entities designated are inferred from human behavior and not from events in consciousness.

If this should be true, it would at once remove a number of contradictions which plague the student of emotion and emotional recognition. The current opinion that human emotion is recognizable only from the nature of the stimulus conflicts with common experience. Also, there is a frequent objection to naming animals' emotions, as something anthropomorphic and unjustified. The attitude is often met in psychologists who do not object in the same way to naming man's emotions. Yet if we accept authoritative opinion, which holds that the name of an emotion is determined either by the stimulus alone or by stimulus and strength of response, naming an animal's emotion ought to be precisely as valid as naming a man's. The original source of such confusion is to be found, I believe, in the general assumption that an emotion is recognized, if at all, from momentary observation; and in the failure to distinguish intrinsically recognizable patterns, such as a temper tantrum, from associated signs such as facial expression which may be diagnostic only when the judge is familiar with the subject.

The persistent idea that the recognition of emotions can be fully tested by allowing the judge to see the subject's behavior for a moment or so, even by means of a picture showing the subject as he was at a single instant, has dominated every experimental investigation of the problem. The results show a low level of agreement among

judges as to what emotion is expressed. Together with a failure to find characteristic visceral patterns for the various emotions, this has led to the conclusion that the subject's actual response has little to do with the name given to his emotion. But the conclusion obviously deals only with spatial or nontemporal patterns of response. All that has been shown is that there is no characteristic pattern of *coexistent motor activity* for a particular emotional state. A further limitation on the conclusion is imposed by the nature of the conditions in which human emotions can be studied experimentally. Uninhibited rage cannot safely be provoked in adults, nor uninhibited fear. It is also hard to obtain emotional behavior that is not suppressed and distorted by social factors. This is not true with infant subjects, but emotional mechanisms at birth are not differentiated as they are later, as is shown by the late appearance of distinctive rage (4) or by the changes of fear susceptibilities with maturation (9, 10, 16, 17, 19). Consequently a failure to recognize infant emotions must be inconclusive. Thus a lack of recognition of emotion has been established only for momentary response patterns, without regard for temporal sequences of integrated action, and has not really dealt with the full, uninhibited expression of human emotion as it is known to occur socially and as one can study it in chimpanzees.

The idea that human emotions are verbal fictions, and that the name given to one is mainly determined by the stimulus situation, seems to have been generally accepted as a result of the failure to find distinctive muscular patterns of response. Sherman's work with infants (18) put emphasis on the stimulus as a factor in 'recognizing' emotions. Landis had drawn attention to this factor in an early paper (11), and more recently has stated his posi-

tion as follows: "In every case it seems that the name [given to an emotion] is one which is assigned to some particular configuration or type of situation" (12, p. 411). Dunlap (6) shares this opinion; emotions are nothing more than 'estimation of the situations in which they arise,' either by an observer or by the subject himself. Duffy (5) includes a reference to the strength (but not the form) of response; stimulus and strength of response are the only factors affecting the identification of emotion. Harlow and Stagner (8, p. 191) adopt a similar position; rage is excitement plus the perception of a 'situation calling for attack,' fear the same thing in a situation calling for retreat.

The consensus, then, is that in any recognition of emotion the only variable, or the most important one, is the nature of the stimulus. This is simply not in accord with common social experience. Human emotions are identified socially without perception of the cause. A wife knows that her husband is annoyed but not what he is annoyed about; a child is seen to be frightened when the observer was not expecting it, although he could perceive the stimulus (the thing he later calls the cause of fear) in advance. If emotions are recognized by situation plus excitement, no one could distinguish fear from rage when a child is punished—the situation is said to provoke both emotions. This difficulty for current theory comes up whenever different emotions are identified in the same situation; when, for example, bawdy pictures produce sexual excitement in one person and disgust in another, or when a practical joke causes fear first and anger when it is repeated. The difficulty cannot be removed by postulating errors in identification. If emotions are only 'estimation of the situations in which they arise,' any one observer would be consistent in attributing one emotion to all

subjects in what he considers the same situation. There are a number of such facts which do not fit the conclusion that the apparent stimulus is the main determinant of recognition. Yet no other conclusion seems possible if man names emotions by a momentary observation without regard to temporal sequences of response: if the classical laboratory experiments provide an adequate test of the recognition of emotion, we are involved in a set of hopeless contradictions.

If, however, it is assumed that human emotions are recognized as the chimpanzee's are, the contradictions disappear. The failure of recognition in the laboratory tests would be accounted for, since human emotions could be named only (1) by perception of a deviation from the base line of ordinary individual behavior, or (2) by the fact that the observer has associated the subject's neutral acts with the intrinsically nameable deviations. Either condition requires an extended period of observation of the subject. The stimulus would still affect the naming: in experimental conditions the observer, lacking all other cues to the nature of the subject's disturbance, would fall back on his knowledge of the species *Homo* and name the emotion such a stimulus would be most likely to produce. Socially, with extended observation of the subject, his judgments would be more confident, less dependent on a perception of the stimulus, and more likely to agree with those of other judges and of the subject himself (although the agreement between observer and subject has certain limitations which are discussed below). Finally, the phenomenon of a named emotion without apparent cause or with an unusual cause would become comprehensible, since the nature of the stimulus would play only a subsidiary role in recognition.

Thus the assumption that man's emotion is recognized in the same way as the chimpanzee's has theoretical advantages. The main objection to it will be found in the common idea that emotions are directly known in consciousness, only indirectly from behavior. This objection must be considered in detail.

VIII. THE SUBJECT'S RECOGNITION OF HIS OWN EMOTION

It is proposed here that the hypothesis of a primary objective recognition can clarify even the subject's recognition of his own emotion. The hypothesis is to the effect that, with human as with chimpanzee subjects, naming emotions is based essentially on observed deviations of behavior. Whenever the subject detects the direction of his own subsequent behavior before an observer does, he can name his emotion quicker and more accurately. He may also have access to 'associated signs' in the form of imagery and so on which the other observer does not have, and yet these signs may originally have acquired meaning from their relation to behavior. The hypothesis therefore does not deny the obvious fact that in some circumstances the subject's recognition of his emotion is superior (quicker, of better predictive value) to recognition by another observer. It does deny that subjective naming is always superior and that it is in essence opposed to objective naming.

The points in support of this view can be listed as follows:

(1) In some circumstances it is accepted socially that the observer's classification of emotions is superior to the subject's.

(2) Authoritative opinion unanimously denies that there is any simple event in consciousness which corresponds to a named emotion, and is clearly in the dark as to the actual

cues used by the subject in naming his emotion.

(3) There is convincing reason to believe that the meaning of emotional terms must have been learned on the basis of distinctions of behavior, hence that these terms have ultimately a behavioral reference.

(4) When the distinction between subliminal and openly expressed emotional excitations is made, it is significant that the superiority of subjective recognition applies only to the first kind of excitation; hence the superiority may lie only in an ability to predict the nature of eventual behavior before observers can, and does not imply that the subject has different criteria of emotion *per se*.

Let us consider these points in order.

(1) Socially, the subject's classification of his emotional excitations is not always trusted. Preliminary signs of sexual excitation in the socially naive (though not necessarily inexperienced) subject may be better evaluated by a sophisticated observer. Jealousy is more apt to be classified as such by others than by the one 'experiencing' it, for to him it is likely to appear as indignation, disapproval on moral grounds, simple annoyance or the like. Subject and observers in a social situation often agree in diagnosis of emotion; but when they disagree do we always regard the subject's report as superior, as a better guide to his future behavior? The answer may be yes, when the observers' reports are variable and disagree; *but when observers agree among themselves and the subject disagrees* it is often accepted that the observers' report is more trustworthy, even when the subject is not lying. It certainly is not assumed socially that a subjective evaluation is always more reliable than one based on the observation of behavior.

(2) Writers on the topic of emotion

insist that there is no characteristic conscious event that can be called an emotion, nor any simple conscious index of an emotion. Dashiell (3), Duffy (5), Dunlap (6), Harlow and Stagner (8), Landis (12), Lashley (15), and Young (21) all deny explicitly or by inference that emotions have any intrinsic conscious content that can identify them. Furthermore, Dashiell, Duffy, Dunlap, Harlow and Stagner, and Landis believe that the main determinant of recognition, even when the subject is naming his own emotion, is the nature of the stimulus. This theory is refuted by the not infrequent reports of fear without cause, subjectively evaluated (*cf.*, for example, Cantril and Hunt, 2), but it means that those who accept the theory see no essential difference in the subjective and the objective recognition of emotion. No other theory of subjective recognition is at present maintained by psychologists. Landis and Hunt (13, p. 484) conclude only that "emotional experience . . . is a highly variable state" and "often partakes of the complicated nature of a judgment." The statement fits closely with the hypothesis of the present paper; taken with Cantril and Hunt's (2) work, in which the injection of adrenalin in normal subjects produced widely varying subjective reports, it indicates that the criteria of emotion subjectively recognized are not constant and possibly casts doubt on the reliability of subjective report. The 'complicated judgment' and variability of report become comprehensible if the hypothesis is accepted that the primary recognition of emotion is in the perception of actual overt behavior; apart from this, it can be seen what doubtful ground we venture on when subjective recognition is made the *sine qua non* of an emotion.

(3) Lashley² has pointed out that as

² Personal communication.

children we can only have learned what names to apply to our own emotions if the emotions can be recognized objectively. Either adults must recognize the child's emotion from his behavior, and supply him with a name for it; or he must first connect the name with something seen in others and then transfer this to his own behavior. In either case accuracy in naming an emotion subjectively is in the first place dependent on a good objective recognition.

Now it is conceivable that the adult is right in naming the child's emotion only 75 per cent of the time, and that the child attaches the name to the conscious process which most frequently coincides with the word spoken by the adult. Because of the adult's inaccuracy in objective recognition, the spoken name would not coincide always with the 'true' conscious processes of emotion, but would do so oftener than with any other mental event. Once this selective association is formed, the child himself would tend always to use the right name for his emotion. Thereafter, his subjective recognition would be more accurate than the original objective recognition by the adult. On this assumption, it does not follow, merely from the way in which the emotional names must be learned by the child, that subjective accuracy in naming is no higher than objective accuracy.

But other facts make nonsense of such an assumption. There is first the extreme difficulty of sophisticated adults in finding any mental concomitant of emotional behavior—shown in the denial that there is one, in the unreliability of subjective report, already stressed, and in the conclusion of Landis and Hunt that the conscious aspect of emotion is complex, elusive and variable. Think now of the freedom with which children express their emotions at the stage at which they would be learning

the meaning of the words 'fear,' 'love' and so on. Would a child first associate the word 'fear' with his own vague, complex and variable conscious events or with his relatively clear-cut behavior? The first meaning of fear to a child must be physical avoidance; of anger, biting and striking; of love, close bodily contact and kissing; and so on. The development of a more subjective and sophisticated meaning will be discussed in a moment, but the facts already dealt with make it necessary to assume that the fundamental meaning of emotional terms must be in their reference to patterns of actual overt behavior.

(4) Finally, it seems that an emotion is better named by the subject than by an observer only when its expression is suppressed and distorted. When fear, anger, jealousy, sexual excitement, affection or disgust is openly expressed, without regard to social standards, the diagnosis is evident to any observer who has a little knowledge of the person displaying the emotion. If it is supposed that man's emotions are named as the chimpanzee's are, the diagnosis of a *suppressed* emotion, however, would require much more familiarity with the subject's habitual modes of behavior; and the one who is most familiar with his habits is the subject himself. If there were no other way of naming one's own emotion but from overt behavior, the subject would still tend to be better at diagnosing his emotion than another observer. Consequently, the fact that self-diagnosis in certain circumstances is apt to be quicker and more accurately predictive of future behavior does not in any way mean that the recognition of emotion is primarily subjective.

However, common experience makes it quite clear that more is involved in the recognition of one's own emotion. Psychologists agree that there is no special conscious quale of an emotion; but

this does not imply that there is no awareness of a tendency toward a certain kind of action, any more than the experimental failure to find specific patterns of muscular action is a denial that a frightened person may run away, or that an angry one may strike. We have then the fact that the subject may be aware of a tendency (desire, intention) to act in a particular way. Obviously he might be aware of such a tendency, when the emotion is socially suppressed, before another observer could be. How such awarenesses are to be explained, psychologically, I do not pretend to know; but it is clear that they exist. Now for the present argument, it does not matter whether one assumes that the awareness of a tendency to run away is perceptually equivalent to running away, or whether one assumes only that the awareness is bound to be associated with the act of running. On all those occasions on which flight is first suppressed and then actually follows, the perceived tendency would be closely followed by the act, and the two would be associated.

Thus the awareness of a tendency to act can be treated merely as an 'associated sign' of emotion, though it may be more. There must be other associated signs which the subject (as he develops from the infantile stage at which anger means only a sudden overt attack) comes to notice in himself as concomitants either of gross behavioral deviations or tendencies thereto. As he grows he is obliged more and more to suppress the frank expression of emotion; and the *tendency* to action, and other associated events, subjectively perceived, must become more and more closely related to the essential meaning of emotional terms. It is not likely that the original meaning, and the relation to overt behavior, is ever lost, but the adult in this culture who is able to

keep out of jail must become used to recognizing his own emotions from perceived tendencies instead of from uninhibited action. Once the overt emotional behavior has come under the full strength of social control, an emotional state is practically always one of conflict between a tendency to action and the processes of repression. In English-speaking countries at least there is no emotion whose expression is unaffected by social standards. The adult subject might easily come to think of emotion as typically being what has been treated here as an incompletely expressed and distorted emotion, as a state of internal conflict between opposed tendencies. Obviously the one observer who knows these conflicting tendencies best is the subject himself, who would also be able to utilize associated signs of imagery and bodily sensation that would be available to no one else. In the civilized expression of emotion, therefore, the person who experiences the emotion would often be in the best position to diagnose and name it even though the assumption of this paper is correct, that the real meaning of emotional terms is derived from overt behavior.

Blatz (1) observed that none of his subjects reported fear in an unexpected fall who had not made abortive movements toward escape at the moment of falling. This directly supports the idea that the subjective recognition of emotion involves the perception of a tendency to action, more or less suppressed. The reports of cold emotion in Cantril and Hunt's experiment (2) show that there are also subjective associated signs of emotion. The reports distinguish clearly between an emotion and its subjective accompaniments. The reports of genuine emotion in the same experiment cannot be evaluated from the data given. The subject's conviction that he actually experienced fear

means either that the injection of adrenalin actually facilitated avoidance responses (experienced as a desire to escape from something, without perceiving the something), or that it produced associated signs which were so strong and compelling that the subject found them a convincing evidence of fear. If one were testing the validity of such reports by the subject, the essential question would be whether the adrenalin actually facilitates avoidance behavior. For the hypothesis of this paper, the fear would be genuine even if actual avoidance were not produced, since emotion is regarded as a state in which a particular deviation of behavior is facilitated, whether liminally or subliminally. The existence of such states could be reliably determined only by a method of summation, and not by subjective report.

To sum up the discussion of the diagnosis of emotion by the subject himself: existing psychological opinion as to the determinants of emotional recognition, and the common assumption that subjective recognition is always more direct and trustworthy than the objective, are both contradicted by fact. All the facts, however, can be comprehended by the assumption that the original reference of an emotional term is to a deviation of observable behavior, and that the growing child learns more slowly, with the possibility of continuing error, how to diagnose his own incompletely expressed emotions. The self-diagnosis is regarded as depending on an ability to detect one's own tendencies toward action before it occurs, and on the association of certain physiological and psychological changes in oneself with overt emotional behavior. In this way the adult could often evaluate his own emotion more quickly and accurately than another observer, but the superiority of subjective recognition would not hold in all circumstances.

IX. CONCLUSIONS AND SUMMARY

The main conclusions of this paper can be summarized as follows:

(1) The recognition of a full, characteristic expression of emotion is the classification of a deviation of behavior from an habitual base line. It is not a discrimination of the momentary behavior itself, but of the direction of the deviation, so that both present and past behavior affect the observer's judgment.

(2) The recognition of emotion otherwise is a discrimination of a state of changed responsiveness detected from 'associated signs': acts which would not have a definite emotional significance in themselves, but which have been observed as the accompaniment of more openly emotional behavior.

(3) The emotions thus detected are inferred special states which facilitate or actually produce the primary emotional behavior of (1), although little is known of these states or of a satisfactory classification for them.

(4) The recognition of emotion in man and animal is not fundamentally different. Conceptually, the states of changed responsiveness discriminated are the same in both cases and have the same relationship to overt behavior.

(5) Even when the subject himself does the recognizing, the ultimate criteria of the various emotions are found in distinctions of overt behavior. In subjective recognition cues are available which a second observer cannot utilize, but these cues (of imagery and so on) must be essentially of the nature of associated signs which the growing child learns gradually to interpret after first learning the meaning of emotional terms in relation to actual overt behavior.

(6) Finally, it may be concluded that the failure to obtain a reliable recognition of emotions in the laboratory experiments of the last thirty years

was the result of a particular experimental procedure (the use of too short a period of observation), and does not show that emotion cannot be recognized socially. Also, the conclusion of some writers that emotions are nothing but figments of the imagination stems directly from the apparent unreliability of recognition; if recognition is reliable socially and does not depend mainly on knowledge of the stimulus, this conclusion is unjustified.

A strong argument in favor of the hypothesis presented is that it removes the contradictions inherent in current discussions of emotion and emotional recognition. A stronger argument perhaps is found in the original datum of this paper: the fact that psychologically sophisticated and unsophisticated alike experience an overwhelming tendency to name the chimpanzee's emotions, with a human terminology. Yet the details of emotional expression are quite different in the two species. Recognition of emotion from the chimpanzee's facial expression is even worse than with man's (Foley, 7), and most of the *incidental* signs of chimpanzee excitement have a significance that must be learned by prolonged observation. Yerkes (20) makes it clear that close familiarity with the chimpanzee is necessary for the discrimination of many emotional states, and that the individual components of emotional behavior do not have the same significance in man and chimpanzee. It is only in the higher-order units that the identity of behavior in the two species becomes evident. Consequently the tendency of any observer to recognize and name chimpanzee emotions is not due to a mere superficial similarity. From my own observations I should say that the recognition of identity really begins only after several weeks of observation, and that the strength of one's

conviction, that the identity is real, increases thereafter for months.

Such a fact might be evidence only of suggestibility, but this cannot be the whole explanation. The behavior of the dog is complex enough, but life-long familiarity with this animal does not produce the degree of 'anthropomorphism' in psychologically sophisticated persons that six months of exposure to the chimpanzee will produce. The tendency to identify human emotional patterns in the chimpanzee is the more convincing since, as I have shown, the identification is not indiscriminate. The naming of attitude or emotion is much less frequent than the reader might suppose, and occurs only when the behavior in its long-term significance and intercorrelations is something already nameable in man. When the elements of behavior have a relation that is not known in man, no name is given to the underlying emotion. The naming, therefore, is not an irresponsible form of animism, but a classification of deviations of behavior. The facts of behavior must constitute the ultimate reference of emotional terms.

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PSYCHOLOGICAL TESTING IN MILITARY CLINICAL PSYCHOLOGY: II. PERSONALITY TESTING *

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In a previous article (13) the authors have discussed some of the implications of military clinical practice for the field of intelligence testing. This second article will confine itself to a discussion of personality tests in clinical practice in the military service. 'Personality testing' is used in a broad sense to include the paper and pencil psychiatric screening devices so popular in this war. As in the field of intelligence testing, there have been no new and novel developments in military personality testing. The picture has been characterized not by the invention of new tests but by the adaptation and refinement of techniques borrowed from civilian practice. This is no aspersion on the ingenuity of military psychologists, but reflects the rapid, healthy growth of personality testing since the last war.

The most prominent aspect of personality testing in World War II has been the development of screen tests for use in neuropsychiatric selection. These screen tests are paper and pencil tests of the 'inventory' type designed for the detection of those individuals who are unfit for military service by reason of some neuropsychiatric condition. They are closely related in spirit to the numerous neurotic inventories in use for many years, although their goal is somewhat wider. Most of the older inventories were specifically aimed at

the detection of 'emotional instability' or 'neurotic tendency,' whereas the present military screen tests are aimed at the detection of *all* types of neuropsychiatric conditions, ranging from the milder behavior disorders through the psychoneuroses and psychoses and including many primarily organic conditions such as epilepsy, encephalitis, and post traumatic disorders (14). While this goal may appear overly ambitious, these tests have proven remarkably successful in military selection.

The number of such screen tests is legion, most military psychologists having at some time or other tried their hand at constructing one; but there are two whose success and wide adoption single them out for individual mention. One of these is the Personal Inventory, often called the PI. This developed from a project of Shipley and Landis, which was subsequently continued by Shipley under the auspices of the National Research Council and later the Office of Scientific Research and Development. The Personal Inventory was developed in close coöperation with the Navy and in one or other of its many forms is widely used in Naval installations. Essentially it consists of a series of pairs of descriptive statements from each pair of which the subject must make a forced choice of that statement which better describes his personality. The other widely used screen test is the Cornell Selectee Index developed by Mittelman, Wolf, Wechsler, Weider and their colleagues. Like the Personal Inventory, there are many forms and adaptations of this test, but

* The opinions or assertions contained herein are the private ones of the writers and are not to be construed as official or reflecting the views of the Navy Department or the Naval Service at large.

in essence it consists of a series of direct questions involving the presence of neuropsychiatric symptomatology. The subject answers 'yes' or 'no' to each question. It has been used by both military services, perhaps more widely in Army circles than in Navy.

In evaluating a screen test it is necessary to take account of two factors—pick-up rate, or the number of unfit individuals detected by the test; and false-positive rate, or the number of fit individuals incorrectly labelled as unfit. In general the better a test is the higher will be its pick-up rate and the lower its false-positive rate. No absolute standards can be set up, as tests that vary widely in pick-up and false-positive rates may each be useful in certain special circumstances, contingent upon the specific purpose for which they are used (11). Depending upon the cutting score which one uses as diagnostic, the above tests have been found to detect from 50 to 90 per cent of the neuropsychiatrically unfit at a cost of from 3 to 25 per cent in false-positives. As used in the military services, such tests are never the final criterion for discharge, but are always supplemented by a personal psychiatric interview. The test merely acts as a coarse screen for selecting men for further intensive clinical study. It is a labor-saving device, directing psychiatric attention where it is most needed.

These tests have proved much more successful in military screening than have similar inventories in civilian situations. This success has surprised some academic psychologists interested in personality tests and testing. To us the answer lies in the simplicity of the problem. Military screening aims at a fairly obvious objective through the use of a simple instrument administered under favorable conditions.

The objective of the Military screen test is to detect unfitness for military

service. This unfitness is a fairly direct function of the existence of certain psychopathological symptomatology. The relationship between pathosis and unfitness is not a one-to-one relationship, *i.e.*, many neurotics may adjust successfully to military service, but there are some neuropsychiatric behaviors which do preclude successful adjustment to service conditions. In civilian life the relationship is not so close and a man may make a successful social adjustment despite handicaps which would automatically disqualify him for a military career. To establish that a man is enuretic is to establish immediately his unfitness for Naval Service, as the social consequences of enuresis in the crowded conditions aboard ship are immediate and disastrous, whereas in civilian life enuresis may exist without disrupting the individual's social adjustment. Somnambulism may be merely amusing in private life. Aboard ship it becomes a constant threat to a man's very life. Severe psychoneurosis, epilepsy, and psychosis are other conditions automatically precluding adjustment in a military setting, but even mild psychosis may exist in a civilian environment without precluding a non-institutional adjustment. The range of behavior permissible in the military services is so much narrower than that permissible in civilian life that our task of selection is made much simpler. Our problem is not that of ascertaining the complex conditions under which a civilian epileptic will fail to make an adequate social adjustment. We have merely to ascertain whether a man is or is not epileptic.

The items on a screen test aim at the detection of symptomatological behaviors such as headache, tachycardia, enuresis, and dizzy spells. They assume such obvious, simple form as the bare questions, "Do you have frequent headaches?" and "Have you ever

fainted?" Here the test item "Do you have frequent headaches?" so closely approaches the criterion, the existence of frequent headaches, that validity hinges merely upon whether or not the subject is telling the truth. Our screen tests are not personality tests in the conventional sense of the term, but merely printed psychiatric interviews.

The simplicity of our measures is further reinforced by the favorable conditions under which they are administered. The military recruit taking the test is under tremendous impulsion to tell the truth since the consequences of lying to military authority may be severe. As a result, the military psychologist often gets the truth where a civilian psychologist would not. Moreover, the recruit accepts the situation in a serious vein. Military service is a serious matter; it is undertaken under strong personal tensions, and these factors also tend toward truthfulness in answering the questions.

To say that our screen tests are not personality tests in the usual sense of the term, to say that they are merely printed psychiatric interviews, is not to belittle them or to obscure their contribution to military selection. They are needed, and they work! The shortage of trained psychiatric personnel makes some substitute such as the screen test necessary. In some instances screen tests may even perform more efficiently than a psychiatrist, since not all psychiatrists are adaptable to the clinical rough and tumble which neuropsychiatric screening entails (21). To mention one advantage that the test does have over the human clinician—the test never forgets. It always asks *all* the questions, while even the best clinician may sometimes be distracted and forget in his interviewing to cover all the possible areas of disability. The outstanding success of screen tests rep-

resents one of the most important testing contributions in World War II.

In our previous article on intelligence testing, we stressed the success that has attended efforts to abbreviate intelligence tests. The same success has attended the abbreviation of screen tests. It was possible to reduce the Personal Inventory to a mere 20 items and the Cornell Selectee Index to 32 and still have tests that functioned acceptably. A combination of these, officially known as Navy Enlisted Personal Inventory Form 2, has performed outstandingly (21). Such abbreviation was associated with only a small drop in the pick-up rate of the tests, although there was a much larger increase in the false-positive rate. The false positives, however, can be corrected by the subsequent psychiatric interview. Such changes in efficiency must be evaluated in the light of the demands of each separate screening situation, and an increase in brevity is often worth the slight decrease in test efficiency that it entails. In military testing the value of any test cannot be determined by fixed and absolute standards. Value is a relative matter determined by the economic factors involved (14).

In reporting the efficiency of such tests, and indeed of many other selection tests used in a dichotomous, pass-or-fail situation, a coefficient of correlation has dubious value since it conceals in a single expression two independently varying measures—the pick-up rate of the test and its false-positive rate. These two aspects of a selection test not only may vary independently of each other but may have differing economic values in the selection situation. Where the test is being used as a coarse screen preliminary to an individual psychiatric interview, pick-up rate becomes all important while the false-positive rate may run as

high as 25 or even 50 per cent without destroying the value of the test. The interviewing psychiatrist can weed out the false positives but he must be sure that most of the unfit are in the group he sees. In such a situation we have found valuable a test which picks up 80 per cent of the unfit although its false-positive rate may run from 25 to 30 per cent (21). The psychiatrist has to see only one-quarter to a third of the group but he still has a chance to identify four-fifths of the unfit individuals he is seeking. Were the selection test results the ultimate criterion and were they *not* checked by a psychiatric interview, manpower economy might demand a test with a much lower false-positive rate although the detection rate were also lower. Yet both tests might show equal coefficients of validity. In such instances the correlation technique with its single coefficient is concealing important aspects of test performance. We favor the use of a 'cost accounting' system in which for each cut-off score a direct statement is made of both pick-up rate and false-positive rate. The further step of calculating a coefficient of correlation is unnecessary.

The Rorschach test has proven to be as popular and as valuable in military circles as it has in civilian. Little that is novel has come out of its conventional military use. With malingerers the response pattern offers interesting and suggestive resemblances to that seen in cases of severe constriction and inhibition (3, 12). Grinker mentions the value of the Harrower-Erickson Stress Tolerance Test (7) in which the subject is presented with five ink blots followed by ten combat pictures. These are followed in turn by five more ink blots. The difference in response to the two sets of ink blots supposedly measures the emotional stresses aroused by the combat pictures.

It was inevitable that attempts should be made to abbreviate the Rorschach technique in order that it might be adapted to the mass production methods of military selection. The best known of these attempts was made by Harrower-Erickson who combined group administration with a multiple choice response situation and an objective scoring key. This is the widely circulated Harrower-Erickson Multiple Choice Test (9). Its use in selection, however, has not been attended with any success. Reports by Wittson, Hunt and Older in Naval selection (22), Due, Wright, and Wright in a Naval hospital (6), Springer on a Naval disciplinary group (20), Jensen and Rotter with an Army group (15), Balinsky in the U. S. Employment Service (1), and Challman in a state hospital (5), have all been negative. In view of this developing body of negative evidence, the continued use of this test is interesting, and illustrates the widely-felt need for an abbreviated Rorschach technique.

While the present authors have contributed to the negative evidence mentioned above, we do not feel that the possibilities of either the multiple choice method or objective scoring received fair trial in the present version of the test. Familiarity with the date of the Wittson, Hunt and Older study leads us to believe that had Harrower-Erickson followed the usual painstaking steps of item analysis and test validation, relying more on empirical investigation and less on a priori judgment in selecting her alternative responses and constructing her scoring key, the results might have been different. It is to be hoped that someone will find time to give this basic technique the complete investigation it deserves.

The group Rorschach technique has furnished only a partial answer to the problem of abbreviation. It results in a large saving in the time necessary for

administration but still requires an individual interpretation of each record. Group administration has been of value in military hospitals and clinics but has not been able to meet the demands of the mass production methods of induction center and training station. The literature continues to show some changes in the type of response occurring as a function of the group situation (16). Such changes, however, do not appear to invalidate the method. All these attempts at adaptation of the basic Rorschach technique reflect a healthy growth and development and suggest the disappearance of the esoteric atmosphere which surrounded the test at first, impeding its acceptance and detracting from its usefulness.

The Thematic Apperception Test has also had some military use, although, as in the case of the Rorschach, the difficulties in administration and interpretation have impeded its application to any situation where time is at a premium. The subjective difficulties of interpretation inherent in the projection technique are magnified in the case of the Thematic Apperception Test by the early stage of its development and the relatively small body of objective data as yet gathered in actual clinical practice (10).

One of the more popular tests of the general inventory type is the Minnesota Multiphasic Personality Inventory which has been used widely in the armed services. The distinctive asset of this test is that it makes available ratings on several different traits or dimensions of personality such as hypochondriasis, depression, hysteria, psychopathic deviate, masculinity-femininity, paranoia, psychasthenia, schizophrenia, and hypomania. Thus it is aimed not only at detecting the deviate but at establishing the clinical direction or directions in which the deviation will be found. This assignment of

scores on a number of varying dimensions of personality, which is the test's distinguishing characteristic, is often overlooked in its validation. Sometimes the Minnesota Multiphasic has been used as a screen test to separate the normal from the abnormal irrespective of the direction the abnormality may take. In other instances the scores or profiles have been correlated directly with inclusive psychiatric categories such as psychoneurosis, sexual psychopathy, inadequate personality, constitutional psychopathic state, etc. (8, 19). Little attention has been paid to the agreement between the score on each dimension or scale and the actual presence of clinical behavior of that specific type. This is brought out by Schmidt's statement "the data are in agreement with Leverenz' observation that although the clinical impression may not be corroborated always by the scores, the clinician is made aware of one or more personality abnormalities that require evaluation" (19, p. 130). When used in this way the test seems to establish differences between normal and abnormal groups, but some further evaluation of efficiency is necessary. In both its individual and its group form, the Minnesota Multiphasic is time-consuming, not too easily scored, and relatively expensive. Before it should be adopted widely for military purposes, it should be evaluated against other tests now available for the same purpose. For use in screening there certainly are available many tests of the Shipley or Cornell Selectee Index type which are more economical and probably of equal if not greater efficiency. For rough diagnostic purposes the Rorschach, while possibly not better on economic grounds, may well be more efficient, although handicapped by the need for trained testers. In the military services where efficiency has a wider connotation than mere validity,

we should like to know not only that the Minnesota Multiphasic is a good test, but whether it is better than the others available.

Benton has attempted to validate the various individual scales of the Minnesota Multiphasic against the psychiatrist's clinical impression (2, 4). This is a necessary direction for future validation of the Minnesota Multiphasic to take. Benton's results show that in general the scales are useful although not of equal validity. In his most recent study he has correlated relative standing on the various scales with the clinical impression of psychiatrists who have been thoroughly indoctrinated in the meaning of each scale. His studies indicate that while some of the scales such as those for paranoia, schizophrenia, and psychopathic deviate are apparently valid, others such as hypochondriasis, depression, hysteria, and masculinity-femininity do not agree well with clinical impression, since the scores on these scales run much higher than the psychiatric judgment of the actual clinical picture. It is interesting in this respect to note that in a recent article on the use of the Minnesota Multiphasic in vocational advisement many of the subjects interviewed scored quite high on the scale for hypochondriasis, depression, and hysteria (8). In view of Benton's results, it is possible that Harmon and Wiener have given undue weight to this finding.

As personality tests go, the Minnesota Multiphasic Personality Inventory would seem to be a fairly good test. Whether or not it is worthy of the wide use which it is rapidly gaining is more questionable. Further study is indicated, particularly toward the improvement of the various individual rating scales.

Malingering on personality tests has been studied. Benton reports a typical malingering pattern on the Rorschach

(3) which is confirmed by Hunt (12). Differences have also been found in scores on screening inventories, although here the differences are quantitative rather than qualitative (12). At this point, however, caution should be introduced against assuming that the presence of malingering on a personality test invalidates the resulting score. In many cases malingering is symptomatic in nature and revelatory of an underlying personality disorder. In such cases, while the man may be brought to clinical attention because of a score which is essentially 'false,' the end result is the detection of a man unfit for military service.

The success of psychological tests in the clinical situation has led in many cases to a regrettable tendency to substitute the psychological test for the clinical interview. In the opinion of the present authors such a substitution is unfortunate. Test and clinical interview are two separate methods of investigation and the best results are obtained when these two methods are used separately but later allowed to complement each other in dictating the final clinical decision. In favor of the clinical interview as a method, we might point out that it covers a wider range of material than any single test, that it is more flexible and can be adapted to the specific needs of any particular situation, and permits an integration of clinical data that the test does not. Hunt, Wittson and Harris have compared the two methods and summarized the advantages of the clinical method by stating that the test cannot *think* while the interviewer can (14). In addition, it should be pointed out that the clinical interview is often quicker than the psychological test. We have seen many cases of schizophrenia laboriously diagnosed after two or three hours of Rorschach administration and evaluation when the diag-

nosis could have been established in a twenty minute clinical interview.

Since the clinical interview is an important tool, it would be a shame if the psychological test in its present state of immaturity should be allowed to supplant it. It is true that the clinical interview cannot be used profitably by everyone. Many clinicians are singularly inept in its use and there are many times when "a good test is better than a poor psychiatrist" (14, p. 45). The ideal solution of this difficulty, however, is not to drop the interview but rather to train clinicians in their use of it. Were the same time and effort now expended in developing psychological tests expended upon the study and development of the clinical interview, much clinical progress could be made. While a good test may be better than a poor clinician or a good clinician better than a poor test, the best clinical results will follow when a good clinician is complemented by good tests.

We have stressed that psychological test and clinical interview are two separate methods of clinical investigation. In their use they should be kept separate and not allowed to contaminate each other. Too often in present military practice the clinical interview is built around and guided by the results of the psychological test. As Leverenz has said in discussing the Minnesota Multiphasic Personality Inventory, "... it has aided greatly in giving direction to the inquiry in evaluating obvious psychiatric disorders" (17, p. 619). Such contamination is unfortunate, as it deprives the clinical interview of much of its value. Only when these two methods are held separate and then integrated at a later stage are the full potentialities of each realized.

As an example of the use of the two methods to complement each other

without contamination, let us mention a typical psychiatric routine which might be practiced in any Naval training station, receiving ship or other installation where psychiatric screening is taking place. The men to be screened first would be given a paper and pencil screen test. Following this all the high scorers on the test would be given a brief psychiatric interview, whose purpose would not be to arrive at a final diagnosis but merely to separate false-positives from the genuinely unfit. The suspected cases of neuropsychiatric unfitness picked up by this procedure would then be referred to an observation ward where they would submit to a longer psychiatric interview whose purpose would be to establish a diagnosis. As a result of this interview, a diagnosis might be immediately forthcoming or, in many cases, it might not. In these latter cases in which the clinical interview is not immediately able to establish a diagnosis, further psychological testing is indicated, perhaps a Wechsler-Bellevue, a Rorschach, or both. Subsequent to this testing a final integration of interview and test results would be made. In Naval practice this integration is the result of a conference between psychiatrist and psychologist.

Mention should be made of the therapeutic possibilities of the testing situation. Even the filling out of a simple screening inventory like the Shipley Personal Inventory or the Cornell Selectee Index offers the subject a chance to become aware of and assess certain areas of personal difficulty. When the results of a Rorschach, Minnesota Multiphasic, etc., are discussed with the patient, it is often possible to direct the interview more easily into productive channels and to hasten the acquisition of insight. This is facilitated by the authority which often adheres to a 'test' in the mind of the

layman. The choice of how much to reveal and when to reveal it, however, demands clinical skill. Such an approach in the hands of an untrained clinician may lead to disaster.

Any teacher who has demonstrated a psychological test through class participation realizes how close such an approach may come to group psychotherapy. Preliminary investigation in the military service confirms the opinion that group testing with group discussion of the results may have a useful place in group psychotherapy. For such purposes many tests like the Rosenzweig P-F Study for assessing aggressive reactions to frustration, while not as yet adequately standardized for general clinical use, nevertheless may function as a basis for a group therapeutic session (18).

In conclusion it should be stated that the size of the psychiatric case load plus the shortage of trained psychiatric and psychological personnel in the military services has resulted in two characteristics of military clinical practice that differentiate it from civilian practice. These are the stress upon speed, and the stress upon classification and disposition at the expense of therapy. These demands have formed and limited the development of military clinical testing, but within these limitations there has been much healthy growth that should serve as a genuine stimulus to post-war clinical psychology.

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A REPLY TO DR. FINGER

BY M. E. BITTERMAN

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Dr. Finger's critical treatment (2) of my recent paper (1) on certain aspects of the problem of abnormal behavior in animals appears to warrant further discussion. Disregarding his introductory comments on the question of 'personal bias,' we may proceed directly to a consideration of the principal points of disagreement.

The first part of Finger's note is devoted to a 'semantic' analysis of the following segment of a sentence to be found in my paper: "... all situations in which abnormal behavior has been observed in animals may be interpreted as *conflictful* . . ." (1, p. 375). Finger criticizes my failure to define the term 'abnormal' and points out that the use of the universal 'all' leads logically to the inclusion of 'electroshock convulsions and deteriorated behavior following cortical operation . . . in this category' (2, p. 230). Considered literally and in isolation, the statement in question is indeed inexact, although most readers probably understood that I was referring to the class of behavior disorders commonly labeled 'experimental neurosis.' I do not attempt to excuse my own carelessness when I contend that in electing to debate the matter Finger invests it with undue importance. The paper was not intended, as he maintains, to provide evidence in support of the broad statement which he criticizes, but to demonstrate the value of an hypothesis of Sears and Hovland for the understanding of conflict situations.

In the remaining portion of his article, Finger presents evidence for his belief that convulsive seizures occurring in rats under certain conditions of stress

'need not be interpreted to be the result of conflict' (2, p. 231). His argument may be stated as follows: For many years numerous experimenters subjected rats to a great variety of stressful and conflictful situations, but no startling patterns of disorganization were reported until Maier described the convulsive behavior in question. Conflict, however, does not seem to be an important factor in the etiology of this behavior, while auditory stimulation does appear to be critical. Only in the case of one rat have seizures been induced in a conflict situation not involving sound; but many animals, failing to respond in such situations, have shown the typical pattern 'after a few seconds of their initial auditory stimulation . . . in the absence of any deliberately planned conflict and even in spite of the experimenter's diligent efforts to exclude the possibility of such a factor' (2, p. 232). Susceptibility to seizures is influenced by a variety of agents and conditions—age, emotionality, diet, drugs, exercise, etc. A few convulsive seizures have even been observed to occur "spontaneously" or as a result of certain cutaneous (or perhaps proprioceptive) stimulation" (2, p. 231) in rats with severe dietary deficiencies.

This argument does not seem to me to weigh very heavily against the conflict interpretation. Let us consider first the implication that auditory stimulation is in itself capable of eliciting the convulsive pattern. There is no denying, of course, that the highly exciting auditory stimulation plays an important part in the process, but—even with the physiological condition of the animal constant—there is no reason to assume

that it is the only significant factor in the experimental situation. The animal thus stimulated invariably is placed in some sort of enclosure—it would not otherwise remain for very long in the vicinity of the sound source—and there is a good deal of evidence to show that with the auditory factor constant the frequency of seizures may be markedly influenced by changes in the degree of restriction imposed upon the animal (1). It must be recognized, therefore, that at least *two* situational factors are significant for the experimental outcome—the excitatory auditory stimulation and the inhibiting physical barriers¹—and these factors may be thought of as constituting the elements of a simple conflict situation. Whether or not other stimuli with the generalized excitatory properties of intense sound will eventually be discovered remains an open question. But even if it could be demonstrated that auditory stimulation is an indispensable condition for the occurrence of seizures, present evidence indicates that it is by no means a sufficient one.²

¹ Empty spaces as well as walls may serve as barriers. It is instructive to observe the behavior of animals placed on a small shelf high above the ground and stimulated in the usual manner. Barriers may be 'psychological' rather than physical. The stimulus cards in a difficult discrimination problem may call out strong avoidance responses.

² The analysis presented here is of course not new. In the second of Maier's series of papers dealing with the problem we find the following statement: "The conflict theory . . . may also account for attacks occurring in the simple situations. In these tests the animal is placed in a restricted area and is exposed to an auditory stimulus which gives rise to general emotional excitement. Since no specific avoidance responses are elicited by such stimulation and since escape responses are sufficiently blocked by varying degrees of confinement, there is a conflict between the excitation produced by the noise and the inhibition produced by walls or edges of a table. The animal is thus forced to react and has no response available" (3, p. 25).

The remaining aspects of Finger's argument provide even less of a challenge to the conflict interpretation. The fact that other techniques have failed to produce such marked disorganization as the convulsive pattern has little bearing on the question. Not all conflicts at the human level result in severe neurotic symptoms, yet there is no denying that some do have serious consequences. It is likely that the absolute and the relative strengths of the conflicting response tendencies, as well as the availability of compromise adjustments, are significant for the outcome. The assertion that various factors such as drugs, dietary conditions, and the like influence the frequency of seizures is equally irrelevant. Although many of the findings with respect to these variables have been contradictory, we may admit the possibility that any number of them may affect susceptibility to attack without detracting from the significance of the conflictful nature of the situations employed. We do not deny the importance of motivational conflicts in the etiology of war neuroses merely because physical stresses such as hunger, noise, and fatigue play a contributory role. The finding that 'spontaneous' seizures may occur in rats with severe dietary deficiencies also is beside the point. Heart rate may be increased by the perception of a threatening situation as well as by physical exercise, yet the efficacy of neither of these factors is denied even though 'common physiological mechanisms' may be involved. Finger also suggests that seizures begin 'after a few seconds' of the 'initial auditory stimulation' (2, p. 232), and it might be asserted that latencies of this magnitude would scarcely allow sufficient opportunity for perception of the conflictful elements of the situation. The papers to which Finger himself makes reference show, however, that mean latencies usually

range from 40 to 65 seconds. The data on variability do, it is true, indicate that attacks have occurred after much shorter intervals. But when may an attack be said to have begun? Some writers have considered the initial tendency to run about the enclosure as part of the attack proper, the latencies reported being computed from the onset of the stimulus to the time of appearance of this behavior. However, since convulsions usually appear considerably later, and since they are often preceded by alternate bursts of running and periods of relative quiescence, the validity of the data on latency may justly be questioned.

To summarize: All situations in which healthy rats have responded convulsively to auditory stimulation may be

regarded as conflictful, since the sound has invariably been accompanied by restrictions—physical or psychological—which limit the scope of the animals' behavior. No argument presented by Finger detracts from the validity of this interpretation, which, in the light of present evidence, cannot be easily dismissed.

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THE PSYCHOLOGICAL SELF IN THE PHILOSOPHIES OF KÖHLER AND SHERRINGTON

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There is in scholarly thought a rule that to some of us has emerged as the first Law of Scholarly Ethics. It can be stated as: *know the implications of your own expressed opinions even to the most distant, or nth, level of discourse.* Many thinkers scorn that law; a select few have lived up to it. The latter are those who have outlined their philosophies of nature. In the history of natural philosophy, is reflected again and again, the interminable conflict: *bias versus fact*; wherever bias is strong, fact deteriorates, wherever respect for fact is strong, bias breaks down. It is quite common that prejudiced scholars repudiate facts that cannot be subsumed under their bias; there are pitifully few instances when a scholar confronting facts humbly admits, despite a bias: *the impossible is true; it happens as a matter of fact.*

Whitehead has warned us against the bigots who allow facts to deteriorate.

"It is easy enough to find a theory, logically harmonious and with important applications in the region of fact, provided you are content to disregard half your evidence . . . people are apt resolutely to ignore, or to explain away, all evidence which confuses their scheme with contradictory instances. What they cannot fit in is for them nonsense. An unflinching determination to take the whole evidence into account is the only method of preservation against the fluctuating extremes of fashionable opinion. This advice seems so easy, and is in fact so difficult to follow" (6, p. 268).

In 1937-38 Sir Charles Scott Sherrington delivered the Gifford Lectures in Edinburgh. These outline his philoso-

phy of nature—a philosophy that instances the rare occasions when a savant frankly admits *the theoretically impossible to happen as a matter of fact.*

Sir Charles proposes that nature contains two kinds of actualities, energy-systems and finite minds. The energy-systems which constitute an uninterrupted continuum hold everything we used to call matter but they hold also all forms of life. There is thus no gap between the inanimate and the animate: life is not a valid category in nature; neither, accordingly, is birth nor death. But the finite minds form an actuality different from the energy-systems; the crux of the difference lies in the latter being energy, the former not.

"Physical science, finds us, like the rest of what it has examined, a mass entirely resolvable into units of what is called 'energy.' . . .

There remains however among the happenings . . . in such a compound organism as ourselves and our like a certain residue seemingly not thus resolvable.

. . . They come . . . under the one word 'mind'" (5, p. 204).

" . . . Mind is not demonstrable as energy; and conversely energy is certainly not demonstrable as mind" (5, p. 206).

"The two concepts mind and energy, . . . cover all our experience, are both of course in themselves creations of thought. But what they respectively stand for still remains divided . . ." (5, p. 260).

The finite minds as well as the energy-systems evolve. The former do not evolve from the latter; rather, when the energy-systems have reached a certain level of complexity of integration—as

in the case with the human organism controlled by a roof-brain—the condition is set for *recognizable mind* (mind knowing itself) to appear. The energy-systems have evolved from elements of their own; recognizable mind has evolved from subconscious (non-recognizable) mind. As mind has evolved on our planet's side, it has at least a temporary survival-value. That means that in some manner it is able to make more efficient man's adjustments in the world to which he belongs. That evolutionary consideration, merged with the message of common sense, leads Sir Charles to the unequivocal dualistic interactionism; the two incommensurables, body and mind, reciprocally act upon each other. The most striking feature of his position is that, though he frankly confesses to that view, he holds it *theoretically impossible*.

"The sun's energy is a part of the closed energy-cycle. What leverage can it have on my mind? Yet through my retina and brain it seems able to act on my mind. *The theoretically impossible happens*. In fine, I assert that it does act on my mind. Conversely my thinking 'self' thinks that it can bend my arm. Physics tells me that my arm cannot be bent without disturbing the sun. Physics tells me that unless my mind is energy it cannot disturb the sun. My mind then does not bend my arm. Or, *the theoretically impossible happens*. *Despite the theoretical I take it my mind does bend my arm, and that it disturbs the sun*" (5, p. 313, italics mine).

About two hundred years ago David Hume wrote:

"For my part, when I enter most intimately into what I call *myself*, I always stumble on some particular perception or other, of heat or cold, light or shade, love or hatred, pain or pleasure. I never can catch *myself* at any time without a perception, and never can observe anything but the perception" (1, I, Pt. IV, sec. 6, p. 534).

Since, in this manner, the psychological self was lost in the flux of sensations and percepts which to Hume constituted the phenomenal world, it has been nearly exiled from academic psychology during two centuries. Not entirely though, for the psychological self continued to live in the thought of the *Scotch School of Common Sense Philosophy* and in the thought of a few partisans of that school down the nineteenth and twentieth centuries. It is encouraging to those of us who have harbored the psychological self as an actuality, to find it reëmerge as a Phoenix in the natural philosophy of a veteran worker in a strictly scientific field, the recipient of the Nobel prize for scientific distinction, and the deliverer of the Gifford Lectures: Sir Charles Sherrington.

Sir Charles's conception of the 'finite recognizable mind' is unquestionably a sibling of the psychological self of the Scotch tradition. Few contemporary psychologists, including those who have achieved a natural philosophy, have retained but a shadow or ghost of a psychological self. How have they compassed without it? Wolfgang Köhler's case is pathognomonic.

Köhler's natural philosophy can be briefly stated in ten predicates of entia.

1. There is a *phenomenal* world.
2. There is a *transphenomenal* world which is the *physical* world.
3. There are in the physical world human brains encompassed in human organisms.
4. Events betide in the phenomenal world, in the physical world, and in that part of the physical world which is the human brain.
5. Between macroscopic physical event outside a brain and brain-event obtains a relative structural-dynamic isomorphism.
6. Between brain-event and phenom-

enal event obtains a perfect structural-dynamic isomorphism.

7. As a consequence, a relative structural-dynamic isomorphism obtains between macroscopic extra-cerebral physical event and phenomenal event.

8. Organismic events of regulation follow largely two laws: the Law of Dynamic Direction and the second Law of Thermodynamics.

9. Macroscopic physical events, physiological events in the human organism including the brain, and phenomenal events, all tend toward simplicity and regularity; this tendency and the two laws of regulation are in harmony.

10. All the isomorphic relations are the necessary consequence of biological evolution.

For our present purpose, only a few of these predicates require a further examination.

Referring to predicate 1, there is a branch of inquiry called phenomenal or phenomenological study. It has been practiced for different reasons by different people. Phenomenal study tries to describe the sheer appearance of things. Unfortunately, phenomenological description seems to leave room to the individual student of exercising private opinion, and so we find a variety of contradictory claims as to the real attributes of the foaming sea of appearances. David Hume exiled from the world of appearances causation. Contrast with his view the verdict of a contemporary.

"... We are aware that what touches or is touched is causally operative on us or we on it. . . . These apprehensions must in some sense be elements of immediate awareness, even if they are quite inarticulate . . ." (4, p. 150).

Köhler, for the sake of his hypothesis of isomorphism (*cf.* predicates 5-7), undertook phenomenological study. He gives us some general advice as to what

we must carefully disregard in order to vision the ens of the appearances.

"... phenomenological statements . . . [should] never be confused with hypotheses or even with knowledge about the functional genesis of phenomenal data. Where a thing has come from, to what its existence or that of its properties is due is . . . not a question for phenomenology. What properties the thing actually has—this is the question of phenomenology" (2, pp. 69-70).

The upshot of his phenomenological inquiries comes to this: phenomena behave in an autogenic, autochthonous, world of their own in which events of segregation and self-distribution of parts result in various contexts. These might be classed as (1) contexts of indifferent belongingness and (2) contexts of requiredness; in the latter enters a phenomenal *ought*. The experienced self is not banished from Köhler's phenomenal world as it was from Hume's. It is reduced, however, to a percept among percepts, the *body-percept*.

Predicate 2/ reads: there is a *trans-phenomenal* world which is the physical world. Describing the physical world as transphenomenal means that it is not directly experienced—it is inferred or constructed. Inference, *qua* experience, is phenomenal, but the content of inference constitutes transphenomenal reality, a substratum underlying the appearances. This distinction has nothing in common with one made by laymen, namely, between *act of knowing* and *content known*; for inference, *qua* experience, is, to Köhler, an autogenic intransitive, a context in an autonomous phenomenal world, not an act intended by a substantive self. In thought *per se* requiredness is inherent; thought is an autochthonous phenomenal event.

One weakness in Köhler's philosophy is that the same argument by which he

claims to establish the transphenomenal reality of the physical world might be used as well—point for point—as an argument in support of the transphenomenal existence of a world of finite minds. His case for the existence of a transphenomenal reality in general is fair enough (2).

"... sometimes concrete reference is a bridge which rests on phenomenal ground on our side, and is still felt, from our side, to have a corresponding support elsewhere [phenomenal transcendence] . . ." (p. 116). "Whenever we try to remember in spite of temporary difficulties, some data are given phenomenally which the thing beyond has to fit" (pp. 117–118). "... the demonstration of any case of transcendence would be a demonstration of transphenomenal reality at the same time" (p. 117).

Thus far, one can endorse Köhler's argument; its continuation provokes my revolt.

"Practically all research in natural science proceeds . . . on the tacit assumption that its subject-matter exists outside the phenomenal world of all observers. . . . Knowing . . . that there is [evidence of transphenomenal reality—the case of obstructed recall] we shall from now on assume that it is evidence of facts which lie within the general realm of physical nature—the same nature with which the scientist is occupied" (2, pp. 121–122).

That argument is biased; abbreviated it runs: (a) directly experienced transcendence constitutes a testimony of the existence of transphenomenal reality; (b) science has always considered the physical world a transphenomenal existent; (c) knowing that there is a transphenomenal reality (term [a]), I assume that reality to be the physical world; (d) directly experienced transcendence testifies the veracity of the physical world (this term is not explicitly given but it is implied in the argument). From term (c) henceforth, the argument is not unequivocally valid;

for, assuming that phenomenal transcendence is evidence of a transphenomenal reality, the assumption that that transphenomenal reality is necessarily of the order of *physical* nature is false; it raises to the dignity of an exclusive what is only an alternative possibility. Transphenomenal reality might as well be of another kind, *mental* reality. Phenomenal transcendence, then, would equally well testify the veracity of a mental as of a physical world. In regard particularly to memory many philosophers have felt that the engram is not an adequate transphenomenal correspondent.

Predicate 8 concerning organismic regulation must be briefly examined.

Köhler suggests that the regulation of living organisms occurs for the same reasons that make physical systems attain an equilibrium. It would then depend upon the Law of Dynamic Direction as well as upon the Second Law of Thermodynamics just as do physical systems. The former law decides what happens in the system, only if sufficient friction within the system eliminates all inert velocities. Friction, no doubt, exists within the organism. That the organism contains many constraining devices is no obstacle to the equilibrium theory; the equilibrium defers to those limiting constraints.

The knot in applying an equilibrium hypothesis to the organism is twofold: (1) the standard state of the organism is not one of the least possible but of maximal potential energy, and (2) the organism is not a closed physical system. Thus we are compelled to adopt 'the conception of a dynamic steady state maintained by a continual expenditure of energy' (2, p. 320).¹

These difficulties Köhler thinks removed if we consider the organism a part only of a larger system including a

¹ Köhler, quoting from A. V. Hill's *Adventures in Biophysics*, 1931.

section of its environment. That larger system closed, the two laws of equilibration would apply to it as a whole. As the organism would be that part of the system in which energy is continually expended, the equilibrium of the total field would want energy perpetually streaming to the organism.

The critical examination of this hypothesis is a task for the physiologist. To the layman a few logical demurs arise. The larger system to which the two laws of regulation are supposed to apply consists, on the one hand, of the organism (O), and, on the other hand, of an extra-organismic section of the world (EO). Accepting these denotations, that larger system in toto would be (O) + (EO). For the theory to work, that system must be closed, that is to say, it must have a boundary through which energy can neither enter nor exit; but for that the two laws would not apply to its internal regulation. The organism, though not a closed system, has a boundary set by its relatively permanent anatomical structure; but, where is the boundary of (EO) or, rather, of the total system (O) + (EO), to which the laws should apply? As long as that boundary is not set independently of the requirement permitting the law to apply, the argument will become circular. In other words, the situation is: the application of the laws requires the larger system to be closed. We assume, then, that the system is closed. But, so long as this assumption is not supported by its own independent criteria, reasoning will remain tautological. The independent determination of the boundary of (O) + (EO)—if boundary there be—would seem difficult.

A. D. Ritchie, examining the application of all equilibrium theory to the living organisms, writes: "Living organisms are never isolated systems, they are always receiving supplies of energy

from the outside, and they are never in a state of equilibrium as long as they are alive" (4, p. 64). There is nothing so far in his statement that Köhler would not endorse. His own hypothesis of the larger field treats the organism as an open system. But Ritchie continues: "The earth as a whole is not an isolated system because it receives energy from the sun. All life on the earth depends upon this disequilibrium at its surface. An organism can live only so long as it has some source of energy on tap to be used when and how it is needed and the ultimate source of all this energy is the radiant energy of the sun" (4, p. 64). These considerations would seem to make inconceivable the outer boundary of (O) + (EO) as well as the outer boundary of (O). If Ritchie is right, then Köhler would seem to be wrong, not only in the logic of his reasoning but also in the factual basis of his supposition.

If I have understood it correctly, the hypothesis of equilibration of the larger field should account for all behavior of the organism. The sound of that broad aspect of the hypothesis requires the examination of Köhler's view of brain function.

Predicate 6 asserts that between brain event and phenomenal event there obtains a perfect structural-dynamic isomorphism. Phenomenal events are field-events. If phenomenal events and brain events are isomorph, then the brain, or at least the cortex of the brain, must constitute a sufficiently continuous medium to allow field-process. The older notion of brain physiology was adverse to that idea; it held the elementary structures of the brain to be isolated, and nervous event in the brain to be an affair of discrete process-conduction. Modern inquiry has largely silenced that objection.

"It is . . . surprising that in the theory of nervous function, and more particularly,

of processes in the ganglionic layers the concept of macroscopic dynamic contexts is so rarely used" (2, p. 207).

"The properties of brain-tissue as a physical and chemical medium are such that in this part of the organism macroscopic contexts of function are not merely possible; it is difficult even to imagine how their occurrence could be prevented" (2, p. 208).

Ernst Mach once made the observation that macroscopic physical states always tend toward a maximal regularity and simplicity. In a regular macroscopic distribution forces balance one another better than in an irregular one. Macroscopic regularity, then, becomes associated with macroscopic stability, and Mach's principle with the self-distribution of processes within the organism. Since all macroscopic physical states follow the principle, the cortical processes should (2, pp. 254-255).

Whether or not strictly in accord with Köhler's views, I venture a reflection. Brain-process is only a part-event in the greater process of regulation of the system we have called (O) + (EO). Nevertheless, in the major event of regulating (O) + (EO), special stresses and strains in the brain would occur. As parts of the brain are leagued with the motorium of the organism, the resolution of the brain-processes can but involve the motor organism. Overt activity would result. In the course of the general bent of the large system (O) + (EO) toward a stable state, that action would contribute to the ultimate regulation of the larger field. This would give us a general theory of action.²

² I believe a passage in Köhler's *The Place of Value in a World of Facts* (p. 358) expresses very nearly the reflection I have independently submitted. My reason for not quoting the passage is that it is too closely associated with Köhler's theory of phenomenal requiredness and its neural correlates to be understood without commentaries too ex-

The stage is now set for our original inquiry: Is there in Köhler's worldview a place for a psychological self? If not, how—as a psychologist and a philosopher—does he manage without one?

The hypothesis of isomorphism (*cf.* predicates 5, 6 and 7) asserts that phenomena concur with dynamic events within certain physical (transphenomenal) systems. These latter always contain at least one living human brain intact in its organism; the extra-organismic section of the system might vary in extent and composition. We are told that the phenomena concurrent with these events, segregate and distribute themselves in a phenomenal field. Insofar as that phenomenal field is a field of percepts, it consists of a phenomenal environment and a phenomenal self. The environment contains phenomenal objects, some relatively stationary, others subject to change. The phenomenal self does not fundamentally differ from other phenomenal things; it is a percept among percepts, notably the body-percept; it can be seen, heard and otherwise sensed; occasionally it is enriched by affective attributes. In one single passage, Köhler makes a reference to something he calls 'our own personality, ourself in a more refined sense of the term' (2, p. 129). We wonder what that might be but we are nowhere informed. Environmental percepts may form various relations with one another. Sometimes they bid for relations with the self; mostly, however, when environmental phenomenal items are salient, the phenomenal self seems to fade. Phenomenal relations may be spatial or temporal; they may be relations of similarity or difference, relations of in-

tensive for the plan of this article. The theory of action remains an utterly vague and sketchy chapter in Köhler's philosophy as well as in his psychology.

different belongingness, relations of requiredness, or what not. Environmental things, appearing as segregated wholes, possess properties which seem to be phenomenally objective. Other phenomenal properties seem to be subjective, that is to say, to belong to the phenomenal self. There is only one single context in which the phenomenal self enters as an active agent. That is in the relational context called requiredness, more especially in the *subjective-objective* type. That type constitutes only one of three, the others being *objective-subjective*, and *objective-objective* requiredness.

"... we shall borrow a term from mathematics and physics and call interest a vector" (2, p. 73).

"Interest as a vector is experienced as *issuing from a definite part of the phenomenal field*. If it is 'my' interest, it issues from that particular part of the field which I call 'myself'" (2, p. 73).

"Interest is experienced as referring to ... a very definite part of the ... field" (p. 74).

"In such organization, ... the vector of interest may be qualified in a great many different ways. All, however, have this in common, that by such vectors the self either accepts or rejects the corresponding objects" (2, p. 75).

"... if and in so far as interest ... is taken in an object, it *acquires* new ... qualities [Gestalt- or Ehrenfels-qualities], viz., value-qualities" (2, p. 78).

Phenomena, then, concur with certain events in the transphenomenal world. They appear and are capable, one would think, of being observed. To whom do they appear? By whom are they observed? As one asks these two questions one feels ridiculous; for common sense has their answer so closely at hand as to make them unwarranted: *to and by some knowing self or mind*. Nevertheless, from the

erudite standpoint of Köhler's philosophy, the questions are all but unwarranted. To bring into focus our problem, I shall discuss it in direct connection with a possible though fictitious situation.

Suppose that a scientific experiment is performed in some laboratory. Suppose that, in the course of the experiment, subjects are asked to give reports upon certain occurrences. One of these subjects is a person (X). The phenomenon to be reported is the (N)-phenomenon, or simply (N). (X), then, observes (N) and reports. So far, common sense. Again, I ask: to whom does the (N)-phenomenon appear? who observes it? and, I add: who reports it? How are these questions to be answered from the point of view of Köhler's philosophy? We will have to try out different replies and examine which ones are possible or preferable from that point of view.

Did the (N)-phenomenon appear to and was it reported by (X)'s finite substantial mind—the kind of mind that Sherrington admits? No, Köhler does not admit of such minds (*cf.* the ten predicates none of which makes any reference to a mental world).

Would the agent observing and reporting the (N)-phenomenon be (X)'s phenomenal self? In one of its aspects at least that supposition would seem untenable. The process of reporting is an organic (speech-motor) affair and so on the par with all transphenomenal events; since no phenomenon could ever be the cause of a transphenomenal event, it could not have been (X)'s phenomenal self that reported the (N)-phenomenon. There remains the possibility of (X)'s phenomenal self observing the phenomenon. In that case, his phenomenal self or body-percept would have been a part of the total phenomenal field emerging in the laboratory. This is not likely. Rather,

was the phenomenal field one in which the environmental event (the (N)-phenomenon) was salient, the phenomenal self obliterated.

Köhler criticizes the literary man, the layman, and even some philosophers for referring all experience to a self. "Percepts are of course not ghosts belonging somehow to the phenomenal 'self.'" "Their place is not in the 'self' . . ." (2, p. 69). ". . . we hear about the 'epistemological subject' who seems to be responsible for the existence of all objects. . . . Phenomenally there is no such entity since the phenomenal self is decidedly not felt to be responsible for the existence of its objects. That other subject is a construct. When we hear about its functions, we soon begin to wonder how different it really is from another construct, namely, the . . . organism" (2, p. 87). Köhler thinks that our tendency to conceive our experience as belonging to or dependent upon ourselves instances a confusion of the phenomenal self with the transphenomenal organism. ". . . all phenomena without any exception are the correlates of somatic processes in the nervous system. To this extent they are . . . *genetically subjective*. . . . In this . . . sense, however, subjectivity means dependence on the *physical organism* and its functions; it does not mean dependence on the phenomenal 'self' or belonging to this 'self'" (2, p. 70).

Referring to subjective-objective requiredness, we can list another reason why (X)'s phenomenal self could not have been the agent observing the (N)-phenomenon. Any admission of such an activity would wreck the experiment. For the entrance of (X)'s phenomenal self into the experimentally produced phenomenal context of (X)'s phenomenal self vectorially pointing toward the (N)-phenomenon with either acceptance or rejection would necessarily distort the latter. In the larger

context (X) — (N) the phenomenal object (N) would acquire an Ehrenfels-quality which it would not possess as a lone segregated phenomenal object.

So far, our inquiry has left us without the (N)-phenomenon observer as well as reporter. Let us examine a new possibility. Could the agent that observes and reports the (N)-phenomenon be (X)'s transphenomenal organism, more particularly its brain? We would have to consider two aspects of that question separately. Verbal reporting is an entirely transphenomenal event. As such it could possibly be effected by the brain. But could a brain observe? The nearest to an affirmation of that question would be the doctrine of epiphenomenalism. Epiphenomenalism holds that experience is the product of nerve activity in certain layers of our roof-brain; it also holds that experience *per se* does not influence natural (transphenomenal) events. If the first of these theorems has any meaning at all, it would seem to assert that the brain is the cause of experience; that the brain is the agent which experiences—a truly useless luxury.

The hypothesis of isomorphism between brain-event and phenomenal event states merely a functional relation (predicate 6). There is, however, no theoretical reason why that hypothesis and the one of epiphenomenalism might not be combined. One would like to know Köhler's stand toward epiphenomenalism. Unfortunately, he is here very evasive. One finds statements which seem to bespeak that he takes epiphenomenalism to heart, but then one strikes passages to the opposite effect. In an earlier article I have quoted a variety of these conflicting statements (3, p. 138). Might it reflect Köhler's position that his foremost follower and collaborator, the late Kurt Koffka, perhaps unwittingly, confesses to epiphenomenalism? Or is his position to be

judged, rather, from his recent book, *Dynamism in Psychology*, in which he is concerned exclusively with the functional relation of phenomenon to brain-event?

Since Köhler foresees any clear commitment on the problem of epiphenomenalism, we shall view his hypothesis of neural-phenomenal isomorphism as a refined form of psychophysical parallelism based upon two substitutions of novel for obsolete notions: on the one hand, the substitution for the idea of discrete process-conduction, the one of the field process, in the brain; on the other hand, the substitution for elementaristic analysis, the one of Gestalt-analysis, of phenomena. From the implication of that position, I shall try to state what would occur in the laboratory during the hypothetical experiment with the (N)-phenomenon.

(1) An event takes place in a physical system (O) + (EO) of which (O) = the organism of (X); (EO) = a section of (X)'s physical environment. The event is the transphenomenal extra-organismic correlate of the (N)-phenomenon; it disturbs the stable state of (O) + (EO). Like all macroscopic physical states, the disturbed state of (O) + (EO) tends toward reestablishment of regularity and stability. This is the essence of Mach's principle; also of the Law of Dynamic Directions, wherever that law applies.

(2) In the process, whether of disturbance or re-stabilization or both, of (O) + (EO), (X)'s brain becomes involved. Within it are projected processes of disturbance and re-stabilization which—structurally-dynamically—are grossly isomorphic with the extra-cerebral processes in the larger system (O) + (EO). This is the essence of predicate 5.

(3) For unexplainable reasons, the (N)-phenomenon concurs with the events in (X)'s brain. Between the

brain-events and the (N)-phenomenon obtains a perfect structural-dynamical isomorphism, that being the essence of predicate 6. In accord with predicate 7 there also obtains between the (N)-phenomenon and the original extra-organismic events in (O) + (EO) a relative structural-dynamical isomorphism.

(4) The brain-events—whether in the phase of disturbance or in the phase of re-stabilization or both—come to involve certain motor 'centers' of the brain; contingent thereupon certain muscles in (X)'s organism contract and relax, muscles largely in the thorax and larynx—causing the emission of air from his lungs in organized sequences of waves ([X]'s report). As these waves are extra-organismic processes, in some curious fashion, they contribute to the final stabilization of the larger system (O) + (EO).

(5) Of the total cycle of events, phases (1), (2), and (4) are entirely transphenomenal (physical). Phase (3) is not; the concurrence of the (N)-phenomenon with the brain events (phase [2]) constitutes the great riddle of all psychophysical parallelism.

To the best of my ability I have attempted to depict what—in the terms of Köhler's natural philosophy—would take place in the laboratory situation chosen as an illustration. It is a weird circle of events. Even more weird does it become when we realize that the (N)-phenomenon, whose concurrence with the brain-events is itself a mystery, appeared to nobody; for, it could not have appeared to any agent unless either one or the other of two conditions were fulfilled: (1) if a phenomenal observer had been a part of the phenomenal field, more especially a part of a phenomenal (Observer-N)-or (X-N)-Gestalt, in which the former part (X) had vectorially pointed to or intended the latter part (N); this was not the case;

(2) if a brain—in our instance the brain of (X)—had been the cause of the (N)-phenomenon, in the epiphenomenalistic sense intending the latter; this possibility Köhler attempts to elude. Therefore, from his point of view the (N)-phenomenon was never observed.

The conclusion is that in Köhler's philosophy there is a place neither for active finite minds of the kind Sherrington regards as one of the two 'essences of nature's content, nor even for contemplative selves who observe without acting. *Nil desperandum*.

Physics has selected from our world of experience essentially such items as can be directly or indirectly quantified. Experimental procedure in that science as a rule sooner or later boils down to pointer-reading. By the use of experimental procedure and of mathematical elaboration of quantified items, the physico-chemical science reached the highly self-consistent concept of the energy-scheme, that vast continuum of energy-systems ever and always in close functional intercommerce. The sciences proper attempted so far as possible to keep man (the psychological self) debarred from their domain. This is how they came to treat percepts as things.

Köhler attempts to parallel in psychology the ways of the natural sciences proper. Like the physicists, reifying their percepts, he reifies the broader phenomenal world. Phenomenal events he describes as due to properties inherent in phenomenal objects *per se*, and thus crystallizes in his philosophy the idea of an autogenic autochthonous phenomenal world. The fact that psychologically all appearances are of double dependence, on the one hand, on a transphenomenal situation and, on the other hand, on an observing self or mind is not admitted. Physicists, not dealing with appearances as a means of

understanding man, could justly disregard that the same double dependence holds for the relatively simple percepts. Psychology, I venture, should not exercise the same disregard. If Köhler's proposal had been merely the matter of a working hypothesis, or, even better, of a method of phenomenal study, there would be little to criticise. It is true, that in order to arrive at what he considers the pure phenomena, he imposes upon himself certain arbitrarily chosen restrictions. Titchener did the same: he imposed upon himself even more severe restrictions than Köhler, believing that, by so doing, he would arrive at the very elements of consciousness. Köhler's principles promise more than did Titchener's principles. A number of his young followers have made interesting phenomenal studies under laboratory conditions. The fact that the majority of these students are completely unconcerned with that hypothesis which is fundamental in Köhler's natural philosophy and so also in his psychology, notably, the hypothesis of isomorphism, does not reflect upon him; it reflects—if it reflects at all—upon the young generation of contemporary psychologists to whom the First Law of Scholarly Ethics is not a law unto them. What, on the other hand, reflects upon Köhler is that, when he extends certain ideas—proper as a working tool—into a natural philosophy, he fails to re-introduce into the scheme of things the psychological self, that self which for relatively narrow methodological reasons could be temporarily—but no more than temporarily—disregarded. Sherrington never committed that error. When, as a physiologist, he labored in his laboratory, he kept the human mind out of his conceptual system but, when he outlined his natural philosophy, he gave it again its indispensable position as a part of nature. For that reason, exactly, Whitehead's

verdict over bigots who reject half of the facts applies to Köhler but not to Sherrington; in Köhler's case, the conflict of bias versus fact, resolves itself into the deterioration of fact; while in the case of Sherrington that conflict resolves itself in Sir Charles accepting, though theoretically impossible, that which *happens as a matter of fact*.

I have outlined earlier Sherrington's dualistic-interactionistic world-view, that philosophy which admits two eminent kinds of actuality in nature, the energy-systems, on the one hand, and the finite minds, on the other; each reciprocally acting upon the other.—A brief description remains of the 'self' or finite mind as conceived by Sir Charles. That 'self' should be compared with the only concept in Köhler's philosophy that is even remotely commensurable with it, namely, the 'phenomenal self' (5, pp. 324–357).

Motor acts, according to Sherrington, bring out the distinction between the self or finite mind and that which is not self. Salient in some of our motor acts—the conative motor acts—is the awareness which relates to the act the self as the agent doing the act. That experience has two sources one of which is plainly sensual. As our muscles contract they provoke sensations and we perceive the act. Perception has always spatial reference and so we perceive the bodily parts which participate in the act. But there is an experience also that is not perceptual, the experience of the 'I-doing.' That experience is not derived from sense. It is the self's direct awareness of itself acting. Unlike the part of the experience which derives from sense, it is not spatially projected.

Perceptual space is three-dimensional. It is apprehended as a sphere in which the self is always central, always more central than what it perceives. The projection of perceived objects is al-

ways in a radiant direction away from the self. The experiences and the doings of the central self are identified as acts of one and the same self; whatever they may be: striving, knowing or feeling, they belong to the self that remains central in its own sphere. The awareness of the 'I-doing' being unprojected, is distinguished from the body. Here, and henceforth, we should recall how Köhler equated the experienced (phenomenal) self and the body-percept, scrapping entirely that other acting but non-perceived self. Sherrington preserves the two, identifying the latter with the finite mind.

The unprojected self is the cause of the projected act; this conjunction is naive, unargued. How do I know the self? If perception means awareness through sense, the self is not perceived; for, there is no sense-organ directed upon it. Unlike my projected percepts which I can observe, I cannot observe the self by concentrating upon it; whenever I try, the self seems to elude me. I cannot, either, ask others to concentrate on my own self as I might ask them to concentrate on some one of my own projected percepts. My self is immediately experienced and entirely private, subjective. To it attaches memory; I can retrospect upon my own subjective experiences; I can also express in language what thereby I harvest. In the same way, other people can express in language their respective subjective experiences. Language, thus, becomes the important means by which we discern finite minds in one another.

The self intuits itself as having a 'whereness' within the body. The 'whereness' is not strictly localized. The self, however, never identifies itself with the body. The body is one of its percepts, like all percepts projected into three-dimensional space. Around its body, projected further away, is the environment.

The body is a coherent object having a definite contour extending all around the self—the latter always being within the contour. The body is peculiar in one respect: it resents encroachment by other spatial objects. Invasion by any one of these gives rise to severe disturbance of the self, the disturbance which is bodily pain. The self intuitively the relation of belongingness of the body to itself. The body is its body, it is termed 'mine'; the self seems aware of itself embodied. While the spatial body has contour, shape, and size, the self has none of these attributes. In this respect, it is the body's antithesis. The body has other properties lacking in the self. The hand, for instance, has color, warmth, smoothness, softness, hardness, weight, and what not; the self has none of these characters. These are all given by sense as attributes of spatial things. No sense-organ being directed upon the self, it lacks even the general spatial attribute of extension; so it is also doomed to be stripped of the special attributes of extended things. Though lacking extension, it has 'whereness.' It is never *there* as the perceived things, it is always *here*; and here means in a vague manner within the body-contour.

The self, at different times, has different salient phases, for instance, acting, feeling, and perceiving. In the last phase especially it finds itself surrounded by space; that fact, however, never gives to it, itself, extension. The self perceives the body and the world outside the body as one single spatial continuum in which it is, itself, the center. The external world, unlike the body, harbors no pain. Unlike the body, which is ever the same body never absent from the self, the things in the external world are many; they come and go, are met with and parted from.

The self is aware of itself not only as *here* but also as *now*; it endures in time. Time, like space, is a continuum,

unlike space, a continuum that moves. Within it are distinguished brief stationary cross-sections, each one of which is a 'now.' Every one psychological 'now' is an integrated whole or unit, the 'self' of the moment. It is a pragmatic 'now,' sometimes longer, sometimes shorter. Conation, one of the states of the self, deals with a 'now' that has yet to come; it deals also with a 'now' that has already been. Memory recalls the latter 'now.' The self, progressing through time,—different of its phases alternately salient—does not lose or even question its own self-identity. Despite change, it is always the same self.

In regard to time as well as space, the self is always in the central position. Compared with things it is largely a negation of all that goes to make things up; yet it is none the less a reality like them.

"If either [thing or self] is more unquestionable than the other it should be the unextended 'I,' being the more immediately established. It has, it is true, disability as regards evidence, it is impenetrably private. A spatial fact . . . can be attested by perhaps millions at first hand. The two therefore possess, as testimony, values of a different order. . . . The 'I' which thinks itself within the body, regarding the body as having within it 'life,' identifies that 'life' with itself. It is the life. The actions of the body are *its* actions" (5, p. 336).

The unbiased reader will no doubt feel that Sir Charles has spoken truly to life. The biased reader may object that what he says is not scientific. The latter verdict could be correct only in one sense, namely, that the mind is incommensurable with our concept of the physico-chemical energy-scheme. That matter is frankly admitted by Sherrington himself. But incommensurability with the energy-scheme does not necessarily make the mind-concept unscien-

tific. If a psychological science, instead of sycophantically living on borrowed concepts, honestly attempts to describe in its own terms what naturally falls within its domain, a description, in these terms, if true to fact, becomes a good scientific description. The finite mind cannot then be condemned *a priori* as Köhler has condemned it. There is a further consideration: not only as psychology proper, but also as philosophic wisdom, the mind-concept deserves a right of way.

There are two logical errors which have been the scourge of human thought down the ages, the one, an error of elation, the other, an error of depression. I am using these terms, *elation* and *depression*, because to me each of the two errors is prototypic of a respective philosopher's neurosis.

Some sciences considering themselves rightly successful pretend that their laws and principles are the sole entia of the universe. And so, in the history of human thought we encounter a variety of biased natural philosophies: physicalistic philosophies, mentalistic philosophies, philosophies even advocating that the universe consists of sheer appearances. Any one of these philosophies has committed the logical error of identifying a part with the whole of which it is a part. No conclusions reached through consideration of only a small number of items, selected from a special point of view, can be properly applied to all the events in that exhibit of abundance—nature as reflected in man's experience

—from which they have been extricated. The error would seem one of superiority-feeling or paranoid elation. There is another well-nigh as severe logical error committed by other sciences. In contrast to the error of elation, it seems an error of inferiority-feeling or depression. These sciences, having been profoundly impressed by the success of a sister science or even merely by her seniority of birthright, have felt themselves humble and inadequate; so, meekly, they have attempted to apply to the natural topic matter of their own domain the laws and principles of that sister science. They have even gone to the length of disregarding in the world of experiences items that by right belong to them because these items have refused to be subsumed under the laws and principles of the successful sister science. Such a procedure naturally leads to hybrid sciences; let us hope that they will be unable to propagate their species.

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PERSONALISTIC PSYCHOLOGY AS SCIENCE: A REPLY

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In a recent number of this Journal Dr. E. B. Skaggs expresses his conviction that 'idiographic' knowledge does not deserve to be called 'scientific' (5). It would not be profitable to dispute this statement of semantic taste, for *science*—a 'purr word,' highly charged with positive affect—is at the present time peculiarly resistant to a dispassionate search for its most appropriate referent. And yet I cannot let Dr. Skaggs' confession of taste pass unnoticed because in stating his preferences he has unintentionally misrepresented some of my own views regarding the methods and theories suited to the study of personality.

He writes, "Now any system of personalistic psychology, such as that presented by Allport, where the effects of learning are stressed so heavily and where individual uniqueness constitutes the data of study, cannot meet the . . . criteria of scientific data or content" (5, p. 237). The criteria for scientific subject-matter, he proposes, are (a) durability in the phenomenon that is the object of the scientists' interest, and (b) the universality of this phenomenon.

My first criticism arises from his inaccurate understanding of personalistic psychology. It so happens that there is only one self-styled system of personalistic psychology, namely that set forth by William Stern. A reading of his *General Psychology from a Personalistic Standpoint* (6) shows that Stern's dimensions (or variables) almost without exception fulfill the criteria of durability and universality. In fact, Stern's writing is as nomothetic as one's heart could possibly desire. Hence, to identify personalistic psychology and

the idiographic outlook is Dr. Skaggs' first serious error.

If he wishes to label my own views 'personalistic' I cannot prevent him, but because of the many differences between Stern's 'system' and my own, I myself would hesitate to accept the label. Stern has prior rights to it. In Chapter 20 of the book that Dr. Skaggs criticizes I have explained in some detail the differences between personalistic psychology and the psychology of personality as I see it (1). Elsewhere I have summarized Stern's views at still greater length and again recorded my criticisms of them (2). Dr. Skaggs seems far more certain than I that I am a 'personalistic' psychologist.

In attacking the idiographic point of view (which, as I say, is not the same as the personalistic point of view), Dr. Skaggs writes, "Literally there would be as many separate psychologies as there are individuals, if we carried Allport's doctrine to the extreme!" (5, p. 237). Such a statement is like saying, "Penicillin is good for everything, including near-sightedness and ingrowing toenails, if we carry the penicillin-enthusiast's view to the extreme." Who wants to carry it to the extreme? Not I. In discussing the proposed distinction made by Windelband and others between the nomothetic and idiographic approaches to mental life, I state explicitly, "The dichotomy, however, is too sharp; it requires a psychology divided against itself. As in the case of the two psychologies (the analytical and the descriptive) advocated by Dilthey and Spranger, the division is too drastic. It is more helpful to regard the two methods as overlapping

and as contributing to one another. . . . A complete study of the individual will embrace both approaches" (1, p. 22). The psychology of personality, I have therefore explicitly maintained, should be *both* nomothetic and idiographic.

Again, Skaggs writes, "Allport, who so severely criticizes the older scientific psychology which dealt with facts common to all mankind, ends up by *abstracting certain general laws and methodologies!*" (p. 236). Why, may I ask, is this so scandalous? Why should not a discipline that is *both* nomothetic and idiographic deal with common laws and methods? For that matter, why should not a discipline almost entirely idiographic in nature, such as history, fine arts, or medical diagnosis, employ as background laws and common methods in so far as these are helpful in comprehending uniqueness?

Dr. Skaggs goes on to state correctly my aspiration when he says that the difference between "his laws and methodologies and those of a Wundt or a Titchener lies in the fact that his are (presumably) more serviceable in giving self-understanding and understanding of other people" (p. 236). Dr. Skaggs is right. I try in my book to offer nomothetic constructs that *improve* upon those traditionally employed. While they are nomothetic in nature, many of them have an idiographic *intent*. To illustrate: such constructs as the theory of individual traits, of the ego, of functional autonomy, of congruence, of the empirical-intuitive nature of understanding, all are generalizations which if adopted would give psychologists greater predictive ability in dealing with single individuals. Similarly, among the *methods* having idiographic intent, and emphasized by me, are the case study, the personal document, interviewing methods, matching, personal structure analysis, and other procedures that contrive

to keep together what nature itself has fashioned as an integrated unit—the single personality. My whole purpose is to show how the psychology of personality can do a better job than it has traditionally done in handling the phenomenon of individuality.

But Dr. Skaggs is not pleased. To him it seems mildly treasonable to suppose that science can extend itself to the phenomenon of individuality. He writes, "If we define personalistic psychology as does Allport [the error here I have already explained] each person is a 'unique individual'" (p. 236). My reply to Dr. Skaggs is that each person *is* a unique individual regardless of who defines what how.

Although I am apparently more impressed than he by the inescapable uniqueness of personality, and by the psychologist's obligation to deal with it, I think I allow adequately for the contributions of the familiar actuarial methods of our discipline. For example, there is merit in the postulation of universal needs and capacities, and in their measurement in the customary language of individual differences. All testing (of the standardized order) must, I fully grant, proceed from the assumption of 'common traits.' Uniqueness in respect to any single variable is known only in terms of its *deviation* from the mean of the standardization group with which the individual is being compared. Where my view is 'unorthodox' is in my contention that psychological science (and I mean here the total course of psychological inquiry) cannot stop with common traits, factors, IQ's, and like nomothetic dimensions, but must admit additional methods and theories to handle the organic inter-relation of the artificialized variables with which nomothetic science deals, and must represent better than it has the personalized coloring of these variables in the individual life. I say in effect: "No doors

should be closed in the study of personality. Abstract dimensions have their place. Let us use them even though they merely *approximate* the unique cleavages which close scrutiny shows are characteristic of each separate personality. Then let us add new methods and concepts where they are needed to grasp better than we have the phenomenon of individual pattern."

The most startling feature of Dr. Skaggs' position is his contention that the biological aspects of personality are legitimate data for science whereas the acquired aspects are not (p. 237). He rules out "such content as ideas, ideals, attitudes, interests, sentiments, purposes, beliefs, ideologies [sic], 'personality traits'" because these "are definitely not common to all people the world over" (p. 235). He maintains that whatever is learned cannot be the object of scientific interest, because learning results in progressive individuality. I daresay that biologists and geneticists would be glad to call Dr. Skaggs' attention to the *unique* inheritance that results from combinations of genes. An inborn temperament is certainly no less unique than acquired habit-systems, and, I suspect, not nearly so universal.

Yet he insists that "any effects wrought in man through experience or learning would be unscientific content for psychology because they would not meet the criteria of durability and commonness" (p. 237). To draw the line here would exclude from the scope of science such pursuits as market research, opinion study, vital statistics, comparative national psychology, humor, custom, much of psychopathology, and, if I understand him correctly, most of the phenomena of perception, memory, judgment, reasoning, language, and motivation, for these are all variable and socially conditioned.

To the purged hall of science he

would, however, admit such constructs as Spearman's *g* and Thurstone's 'basic factors' (p. 235). Yet Tolman has shown clearly that nothing is more culture-bound than precisely these factors. Who, knowing the type of culturally conditioned test-matrix from which factors emerge, can deny Tolman's conclusion that "it is quite obvious that this London (or New York) *g* would not carry over, as such, to the Trobriand Islands"? (7, p. 2). And I question whether Dr. Skaggs is on safer ground with his contention that learning theory, or the Weber-Fechner law, is of eternal and universal applicability, for the operation of both, I suspect, is so basically dependent upon culture-bound and personality-bound interests, that the purely biological component is not only impossible to isolate, but worthless when isolated.

The author insists that "science as we know it to-day, thinks in terms of millions of years" (p. 238). If this be so, I doubt that any biological or psychological discipline qualifies unless its subject matter be trilobites or something else equally remote from human concern. The author's insistence upon durability and universality in the phenomena under investigation would, it seems to me, disqualify nearly every psychologist now at work. Oddly enough, Stern, the personalistic psychologist, would qualify as well as anyone, for his dimensions for the study of mental life are highly abstract and in themselves nearly content-less.

It is much more customary to define science as that form of knowledge that enhances our *understanding*, *prediction*, and *control* of phenomena above the level achieved by unaided common sense. Elsewhere I have argued that in order to attain a higher degree of scientific power thus defined, psychology would do well to adopt the idiographic orientation of its work (3). For in

matters of mental life *understanding*, *prediction*, and *control* are likely to be more complete when the single organism is understood in its own special uniqueness than when exclusively nomothetic (actuarial) probabilities are applied. Although this point reaches beyond the scope of Dr. Skaggs' paper I mention it here because, if I am correct in my analysis of the situation, then according to this *more usual* definition of science, idiographic knowledge fully qualifies for a place of honor.

True, this claim that understanding, prediction and control of personality are better achieved under an idiographic than under a nomothetic mode of approach, has been disputed. But in principle, at least, the hypothesis can be submitted to experimental testing. I have already cited some evidence in its favor (3, p. 34), Sarbin some against (4). The subject is not yet closed. I shall not discuss it more fully here, because it digresses too far from Dr. Skaggs' argument.

In the last analysis his position, I regret to point out, turns on a struggle for status, the motive being revealed in several passages:

"Allport takes a bold stand for the broadening of the concept of science. This may be the proper progressive stand to take but we doubt that our fellow scientists in physics, chemistry, and astronomy will be very receptive to the idea" (p. 234).

"While the study of attitudes, beliefs, habits and skills may be of immense practical and theoretical importance, such studies are not science in the eyes of our colleagues in physics, chemistry and astronomy" (p. 237).

"Our colleagues in physics and chemistry might, and probably would resist any such change in the scientific concept" (p. 238).

"When a colleague in physiology or chemistry tells us that our data are not

scientific, we become rather upset" (p. 238).

"We all want to bask in the light of the great Sun-God Science" (p. 238).

In short, psychology, the climber, must not offend those who have arrived. If it does so, it won't make the club.

This logic of appeasement has little attractiveness for me. Prestige for psychology will scarcely be won by aping those who, at this particular moment in the world's history, enjoy exalted status. Rather, when psychology has ripe wisdom to offer concerning the development of human personality, whether it offers it in a nomothetic or idiographic manner (or both), it will then merit the high position which Dr. Skaggs covets for it.

Though I have disagreed with him in his interpretations and outlook, I am grateful to Dr. Skaggs for initiating a discussion of such basic issues in the study of personality, and for the opportunity he has given me to clarify some of my own views on the subject.

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the first of these was the establishment of a permanent government for the colony. This was done in 1687, when the Massachusetts General Court passed a law which provided for the election of a governor and council for a term of seven years. The second of these was the establishment of a permanent court system. This was done in 1687, when the Massachusetts General Court passed a law which provided for the establishment of a superior court and a court of common pleas. The third of these was the establishment of a permanent system of taxation. This was done in 1687, when the Massachusetts General Court passed a law which provided for the establishment of a system of taxation which was to be levied on the land of the colony.

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